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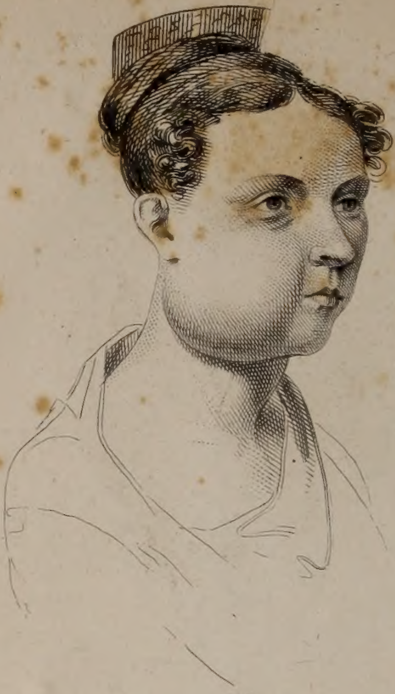

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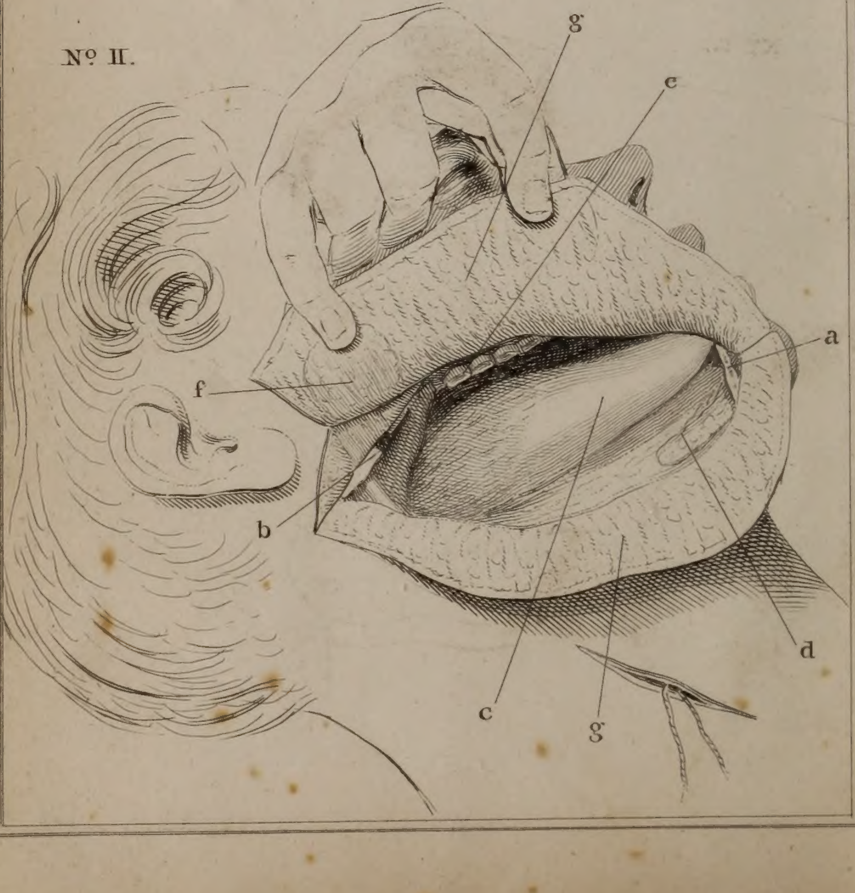

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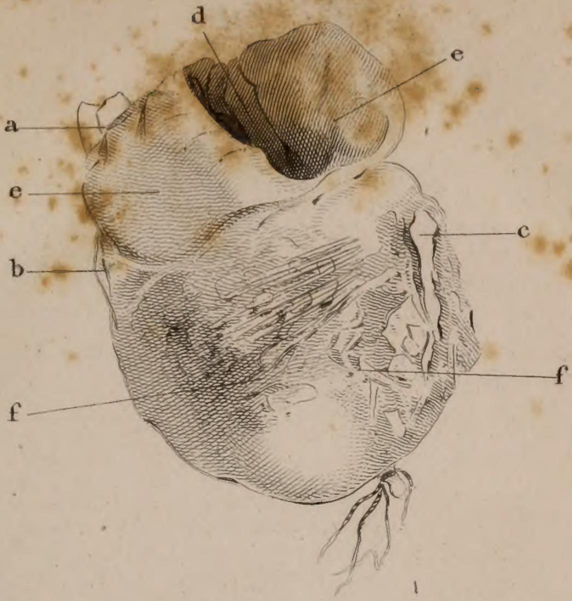

Nº IV.

THE
NEW-YORK
MEDICAL AND PHYSICAL
JOURNAL.

EDITED BY

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PROSPECTUS.

THE rapid and brilliant improvements which have latterly marked the progress of Medical and Physical Science, have directed to its prosecution a degree of attention and zeal hitherto unprecedented in the history of the human mind. For these propitious results we are chiefly indebted to a more judicious method of inquiry than was known to our ancestors, and in no small degree, to the abundance of labourers, who have recently entered into this most extensive field of investigation. In no age have greater talents been enlisted in the cause of philosophy; and, in their application, results are promised, more flattering to the powers of the human intellect, and more auspicious to the interests of mankind, than have yet encouraged the diligence of learning, or rewarded the enterprize of genius. It is honourable to our national character, and gratifying to patriotic pride, to reflect, that to these great objects American talent and zeal have largely contributed.

With a view to aid, by our humble labours, the cause of Medical and Physical knowledge; to furnish a convenient channel of communication to the cultivators of science; and to preserve every fact which may conduce to rational inquiry and practical improvement, the present Journal has been projected. Pledged to no party or sect, while the Editors will offer, with freedom, their opinions and observations on the different subjects which fall within their province, they will, with entire candour and impartiality, afford the same means of publicity, by their pages, to those whose opinions may differ, however widely; and it may be proper to add, that several gentlemen eminent in science, whose theoretical views are entirely opposite, have promised their hearty co-operation and aid.

This work shall consist exclusively of original materials, and will be divided into three principal parts:

1. Original Communications on Medicine and the collateral branches of Science.
2. Reviews of Original Publications. This portion of the work to be devoted to a critical examination and analysis of American Medical and Philosophical writings.
3. Medical and Philosophical Intelligence. In the composition of this portion of the Journal, the principal new publications, both of Europe and America, will be carefully searched, and every improvement diligently and promptly noted.

JOHN W. FRANCIS,
JACOB DYCKMAN,
JOHN B. BECK.

☞ Communications, addressed to E. Bliss & E. White, No. 128 Broadway, or to either of the Editors, New-York, are respectfully solicited.

NEW-YORK

MEDICAL AND PHYSICAL

JOURNAL.

Vol. I. JANUARY, FEBRUARY, AND MARCH, 1822. No. I.

Art. I. *Observations on* Phlegmasia Dolens.

By John W. Francis, M. D.

Phlegmasia Dolens must be enumerated among those diseases whose causes recent inquirers have laboured to ascertain without arriving at a conclusive decision. The complaint is of comparatively rare occurrence, and its connection in most cases with the puerperal condition, has induced writers to adopt an hypothesis respecting its origin too exclusive, and as to its nature unsatisfactory. The theory of the earlier authors,[*] which ascribes it to a metastasis of the milk supposed to be deposited in the cellular membrane of the pelvis and inferior extremities, instead of being carried to the breasts, is, at the present day, almost universally rejected; and the latest observation and experience have furnished numerous facts inconsistent with the ingenious theories advanced by White, Trye, Ferriar, and Hull.

It is now generally admitted, that phlegmasia dolens is a more common affection than was formerly supposed. I am also of opinion, that the disease often exists independent of the puerperal state; that there is less uniformity as to the period of its occurrence, and in the manner in which it invades the system, than is for the most part described; that other parts than the inferior extremities may, under particular circumstances, become its seat; that it is not unfrequently a consequential disease, both various in its causes and formidable in its character; and that it is susceptible of a more active and vigorous method of treatment than is generally recommended.

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[*] Dioscorides; Levret; Mauriceau; Sauvages, Nosolog. Method. tom. ii.

In the cases of phlegmasia dolens which have fallen within my own immediate observation, I have not been able to trace any particular connection between the occurrence of the disorder and a rheumatic diathesis, or that rheumatism had at any time preceded the occurrence of that complaint. In the practice of others I have repeatedly heard of this association. There is unquestionably in many cases a connection between the puerperal state and phlegmasia dolens; yet the latter has taken place where the former had not existed; or where the interval was so great as to forbid the presumption of any relationship. Mr. White seems to have proved that those females who do not give milk are more obnoxious to the disease than those who nourish their offspring: yet this singular complaint happens where the mammary discharge is neither obstructed nor diminished. In the instances which I have seen there was neither suppression of the secretion nor of the flow of milk. Indeed, such suppression I consider rather as the effect than as the cause of the disease. The same observation applies, and with equal force, to derangements of the lochial discharge. Too many cases are recorded to the contrary to admit that the reflux of the lochia had any agency in causing phlegmasia dolens; for where the lochial evacuation was perfectly natural, the complaint has exhibited itself.

Cases of phlegmasia dolens have occurred in which the disease has transferred itself, in the same lying-in, from one limb to the other. The same individual is liable to its attack a second time in the same limb; and in the history of a patient communicated to me by Professor Macneven, it will be seen that the same individual was four times afflicted with this distressing complaint in four successive labours. At each recurrence it attacked the same limb, the right, and was characterized by all the diagnostic signs of the disease.

I shall here insert the account which I have received from Dr. Macneven, of this remarkable case.

" Mrs. M. has had four attacks of phlegmasia dolens in four successive confinements. In the first attack it invaded the hip, and was of long continuance. This was before I came into attendance. The second began on the inside of the thigh, near Poupart's ligament. The third chiefly held the calf of the leg; and the fourth was again in the same part of the thigh. In no case was the pain very acute while the limb was in a state of rest, nor was motion quite impeded but when the calf of the leg was chiefly affected. The whole limb was swelled and tense.

" I learned from Mrs. M. that in the first attack of this disease her physician used fomentations and frictions.

" During the second I employed the anti-phlogistic treatment; and as soon as I had considerably reduced the febrile symptoms, directed the use of gentle frictions, with camphorated spirit and laudanum, together with the use of a flannel roller from the toes to the hip upward. She recovered in about a month. Upon the third occasion the pain was first felt at the groin; but on walking a few steps across the room, it suddenly shifted to the calf of the leg. From that moment all motion was impracticable, and the disease becam emore acute than upon any former occasion. I gave an ounce of spirits of turpentine. It produced considerable irritation, and speedily mitigated the pain; and in forty-eight hours the patient was decidedly convalescent. I had recourse, nevertheless, to the bandage and frictions which had given so much comfort in the former attack, but rather to prevent evil than from any actual necessity. As the lady was remarkably fleshy, she was constrained to wear drawers in order to give an attachment to the roller at the top of the thigh.

" The fourth time, which was during the last winter, I enjoined upon my patient confinement to her bed, kept her bowels constantly loose, gave her water gruel and low diet, and under this management for ten days, she entirely escaped. Every former access of the complaint was within this limit of time. Thinking herself now secure, she began to walk in her room, and after a few turns, had some slight intimations of the disease. I recommended that she should keep to her arm chair, have the leg horizontally supported, use the friction and bandage; through which means she was in a few days perfectly well."

It is not within the experience of any of my fellow practitioners, that I have learned, that phlegmasia dolens has succeeded to abortion.

Dr. Dickson, of Clifton, in England, has furnished us with an extraordinary instance of death from uterine hæmorrhage supervenient to phlegmasia dolens. A woman, on the fourth day after delivery, was seized with rigors, pain in the foot and leg, which afterward extended to the thigh and groin. With the intermediate symptoms and treatment he was unacquainted; but on the tenth day of the attack a practitioner was sent for, who directed twenty leeches to be applied to the afflicted limb, which was nearly twice the bulk of the other. The bleeding, he adds, continued after their removal for the greater part of the night, and on her attempting to get up, a flow of blood suddenly escaped from the uterus, which proved fatal.

Dr. Hull informs us, that he has seen cases of phlegmasia dolens which have terminated in suppuration, as also in death. Dr. Mann, of Boston, was called in consultation to a case of this disorder, where the attending physician, supposing the swelling contained some fluid which required an evacuation, had, previous to the visit by Dr. Mann, made an incision through the skin and adipose substance of the enlarged limb, for the purpose of discharging the contents of it. "Herein," adds Dr. M. "he was not only disappointed, but to his mortification the incised part immediately became gangrenous; a sphacelus of the whole limb soon followed, which terminated fatally in about ten days after the operation."*

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* Communications of the Massachusetts Medical Society, vol. ii.

The disease has arisen after the system has been prostrated to an extreme degree by uterine hæmorrhage. The particulars of an instructive case of this kind I have received from Dr. Handy of this city. Fifty-three days after excessive flooding, and just as the patient was slowly convalescent, a swelling of the left leg suddenly appeared, from the hip downwards, characterized by a white, elastic, tense, and shining appearance, with the other concomitants of phlegmasia dolens, excepting, fortunately, extreme pain and tenderness of the diseased limb when touched.

I shall now enumerate some of the leading circumstances of the cases of phlegmasia dolens which have come within my own observation; purposely rejecting minute details, as unnecessary or superfluous.

The first case I saw of this disease occurred in 1811, in a female of about the middle period of life. There appeared to be no connection between it and the condition of the lochial evacuation. The swelling took place on the fourth day after delivery. In the summer of 1814, I was requested by Dr. Scudder to visit, in consultation, a patient whom he had delivered about two days previous. She was the mother of three children, and had not before been affected as at this time. Her last labour had been in no wise painful or tedious, and the state of the lochia and of the mammæ was natural. The disease in this case was not preceded by rigor or shivering, and began at the lower part of the calf of the left leg, and had, at the time of our visit, extended to the superior part of the thigh; and such was the rapid progress of the swelling, that the whole limb was distended to double its ordinary size. The sufferings of the patient were exquisitely severe whenever she made any exertion to move herself, or upon the least pressure being made upon the extremity. Symptoms of active febrile irritation accompanied the disorder.

In November, 1820, I saw another formidable case of this disease. In this instance, the characteristic marks of the affection did not show themselves until fifteen days after parturition in a first labour. The subject of it had so far recovered from her lying-in as to remove to a distant part of the city as her intended residence. She had a free supply of milk, suckled her own child, and nothing untoward took place as to the uterine discharges. Neither in this case, nor in the two preceding, did there occur that limited swelling of the labium of one side which Mr. White considered as constituting the principal diagnosis of phlegmasia dolens, but evidences of strong vascular excitement and inflammatory action manifested themselves.

In these two latter cases a like plan of treatment was adopted. Copious venesection, free evacuations from the intestinal canal by calomel, or the saline cathartics, occasionally a solution of the antimonium tartarizatum, or small doses of the oxydum antimonii cum phosphate calcis, were administered, and fomentations of tepid vinegar and water applied to the limb. After the active stage had subsided, the patient's strength was supported by the moderate use of nutritious diet and the application of the volatile liniment and flannel roller. In the second case, rigidity of the diseased limb continued several weeks after the removal of the affection.

I have already affirmed that phlegmasia dolens may and does, occasionally, arise independently of the puerperal condition. In fact, the disease is sometimes so little connected with the female functions, that we find the other sex not exempt from it.

I shall state the particulars of an attack of this disease, which I witnessed in 1814, in a young man, who had previously suffered from a long protracted typhus fever. The swelling was first observed in the left groin, which created pain upon motion, and when the limb was at all extended. After remaining several days in this condition, he was accidentally exposed to a stream of damp cold air: within twenty-four hours after this, the leg and thigh were swollen to twice their ordinary size. The skin was smooth, tense, and elastic, and of a shining white appearance; the whole limb was painful to the touch, with great heat of the part, and febrile irritation of the system. This enlargement continued about two weeks, when, by the diligent use of stimulating applications and by bandages, the patient was enabled to walk with the aid of a cane. Dr. Purdy, the subject of this case, has published a fuller account of it in the first volume of the Transactions of the New-York Physico-Medical Society.

Dr. Heermans, of Ontario county, state of New-York, in a letter to Dr. J. B. Beck of this city, has detailed the history of a case of phlegmasia dolens in a young man aged nineteen; and so far as a single instance can be brought to militate against a general rule, it furnishes conclusive evidence that the superior as well as inferior extremities may become the seat of this disease. The patient was subject to rheumatic affections, and had been exposed to inclement and rainy weather some days before his illness. The symptoms of the disorder first exhibited themselves in the calf of one leg, and rapidly extended to the groin, with increase of pain and inability to move. His sufferings were so acute that he was unable to bear the slightest pressure or contact with the skin. At the lapse of thirty-six hours the limb was enormously distended, and had acquired the glabrous aspect and other pathognomonic symptoms of this striking affection. A similar swelling soon commenced in the other extremity; it began at the groin and descended in this leg as rapidly as it had ascended in the other, with the same sensation and appearances. The swelling now continued in both legs down to the extremities of the toes. On the fourth or fifth day, according to Dr. Heermans, the patient complained of the same kind of pain and swelling of the parts about one of the shoulders; but it did not diffuse itself with the same violence and rapidity as it had done in the lower extremities; in like manner the other superior extremity was assailed; after ten days from the first attack, the swelling and distress began to subside in the order in which they commenced; with the exception of the left arm, which continued distended seven or eight weeks, before it was reduced to its natural size. The disease was treated by active depleting remedies, frictions, and fomentations.

Mr. B. aged about seventy, had enjoyed for many years excellent health: after considerable pain and lameness, for some time, of a rheumatic nature, extending down to the foot without any known cause, he was attacked suddenly, with a general swelling of the right lower extremity, attended with fever, and considerable tenderness to the touch. It had the smooth and shining appearance of anasarca, but to the feel it was much harder and more painful. At no time was there the least redness about the limb; for several months at least this was the condition of it after the commencement of the disease. The glands of the groin were considerably enlarged in a short time after the swelling began, particularly the upper row. After the pain and tenderness had in a great measure subsided, evidences of a watery effusion became evident in the cellular substance about the foot and ankle. For several months, however, it remained without the least evidence of water in the cellular membrane.

From the aspect and circumstances attending this case, Dr. Mott, the attending physician, was early impressed with the idea that it was an affection similar to the phlegmasia dolens. The effusion of water in the cellular membrane, about the ankle and foot, must be considered as the sequelæ of the disease, arising from vascular debility. From this result, occasionally attending phlegmasia dolens, the disease has been erroneously considered by some as a species of dropsy.

I shall here briefly notice one other case of phelgmasia dolens, afflicting a male. The patient was a subject of the New-York Hospital, during the winter of 1821-2, and under the care of Dr. Hosack the attending physician. He was aged about forty years; was by occupation a sailor; and, at the time he entered the Hospital, was greatly debilitated by an obstinately protracted remittent fever. Phlegmasia dolens seized his left leg and thigh, which became greatly enlarged, painful, and glabrous; extensive suppuration took place in the thigh, which finally burst, and discharged a large quantity of purulent matter: at the end of two weeks, it eventuated in death.

This termination of phlegmasia dolens in suppuration, rarely happens. A more common result is in a permanent enlargement of one or other of the extremities. In the Transactions of the Medical and Chirurgical Society of London, Mr. Chevalier has described a case of disease occuring in a female aged forty-six years, who had laboured for fourteen years under an enlargement of the right lower extremity, and in which an anomalous alteration was produced in some parts of the cutis; the bulk of the limb depended upon an extraordinary growth of the skin and adipose membrane. The swelling first arose after a labour, which it appears was succeeded by an attack of the œdema puerperarum, which at length somewhat subsided, but never completely disappeared. After she began to go about, the tumefaction again augmented and extended higher; and from that time it gradually continued to increase in bulk, and to become more painful. After the disease had existed nine years, it became so unwieldy as to prevent her walking. At the time Mr. Chevalier saw her, it measured three feet six inches round the thigh, and two feet round the calf of the leg. The whole length of the limb measured, from little above the great trochanter of the thigh bone, three feet three inches: at this part the enlargement terminated by an almost definite line. At the top of the foot the skin was so immensely expanded, as to project several inches beyond the toes, overhanging them so that all except the great toe were imbedded in it. "On the under surface of the foot and toes, and also on part of the upper surface, the skin yielded irregularly, and in some parts it was elevated into tubercles which were exquisitely sore, and many of which from time to time withered at the base, and dropped off. The cuticle was every where thin and chapped. In some parts, particularly at the bottom, it was sodden, being continually moistened by a serous fluid, which was discharged from almost the whole surface of the limb, and which, by its quantity, greatly impaired the patient's strength. The whole surface was inflamed and tender, though not so much as the magnitude of the disease would have led one to apprehend. The left leg," adds Mr. C. "was not at all enlarged in any point." About a fortnight before her death a small portion of the skin sphacelated, and a considerable quantity of serous fluid drained off from the cellular membrane, by which the size of the limb was in some degree reduced. Her sufferings terminated in death.

I have ventured to introduce thus much of this case in this paper, as an example of one of the terminations of phlegmasia dolens, and because it will serve as a caution in our prognosis. Besides, I have seen two other cases of a similar kind. In the one the patient was an indigent female in the New-York Almshouse, aged twenty-five years, who, as nearly as could be ascertained, had been delivered of her first and only child about five years before. The enlargement took place some few weeks after her confinement, which gradually increased to almost the same size as in the instance just stated; it also was exquisitely painful to the touch, and assumed much of the same external appearance. The other was a patient of St. George's Hospital, London, in the winter of 1816, under the oare of Mr. Brodie. I have not been informed of the event of either case.

Perhaps the foregoing facts may tend to elucidate the pathology of phlegmasia dolens. I forbear at this time to enter more fully into the subject. Future observation must assign to the disease its proper rank in nosological arrangement.

New-York, March 4, 1822.

ART. II. FACTS *and* OBSERVATIONS *relating to the* YELLOW FEVER *which prevailed at the Quarantine Establishment at Staten Island, New-York, during the Autumn of* 1821; communicated to the Editors: By JOSEPH BAYLEY, M. D. Health Officer of the Port of New-York.

THERE is scarcely any subject of greater interest to the inhabitants of our cities on the Atlantic, than that of yellow fever. Whenever this pestilence prevails, our commercial prosperity is retarded at home, and embarrassed by long and expensive quarantines abroad. Many valuable lives are lost to the community; many families are left to deplore the premature death of their relatives, on whom they depended for protection and support. Such are the real and not fanciful consequences which result from this fatal fever, whenever it prevails in populous places. It is, therefore, an incumbent duty on every person, whose situation affords him the opportunity, to investigate this subject, so that a number of well attested facts may be collected, in order that we may, if possible, discover the best means of preventing the recurrence of this formidable scourge of our seaports. With this intention, the following facts and observations on the origin of yellow fever at the Quarantine Establishment at Staten Island, during the last autumn, are communicated.*

The violent gale which occurred on the third of September, drove on shore about thirty vessels which were lying at quarantine, and as several cases of yellow fever took place a few days after that event, some persons, perhaps, may be inclined to attribute the disease to the damaged cargoes of those vessels; but it will hereafter appear that it cannot be ascribed to that cause: and the cases of the Rev. Mr. Hardenbergh and Seth Waite, (particularly the former, who was not there during or after the storm,) are an evidence that the contagion of yellow fever existed at the United States wharf, anterior to the gale. For this reason, we will first examine into the circumstances of those vessels which were at that place before the gale. They consisted of the ships Amphion, Ann-Maria, Lucy-Ann, and Le Herondelle, all from the sickly port of Havana; the brig Lion, from a cruise, and the schooner Betsy, from Wilmington, N. C. (at which place a malignant fever was raging, when the Betsy sailed.) They arrived at Quarantine between the 14th and 26th of August, and were brought to the United States wharf between the 23d and 31st of the same month.

The ships Ann-Maria and Lucy-Ann sailed from New-Orleans, in stone ballast, the beginning of June, for Pensacola, and transported from the latter place about six hundred Spanish troops to Havana, who were on board eleven days, and were all landed in health. Those vessels remained only five days at Havana, and their crews continued healthy until they sailed from it; although the yellow fever prevailed there at the time. On their passage from Havana to this port, one seamen died of yellow fever on board of the Ann-Maria, August 6th, and one on board of the Lucy-Ann of the same disease, August 13th. Some of their seamen were indisposed on the passage from the West Indies, and the mate of the Lucy-Ann was convalescent from an attack of yellow fever, when the ship arrived here, August 20th. These vessels remained in the stream from seven to eleven days, during which time they were ventilated, and their forecastles and holds were white-washed. The ballast of the Lucy-Ann was overhauled, and the place where it lay was also white-washed; but nothing had been done with the ballast on board the ship Ann-Maria, since she sailed from New-Orleans.*

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* For the benefit of distant readers it may be proper to state, that the Quarantine Establishment is about six miles below the city, on the west side of the Bay of New-York. *Ed.*

The ship Amphion arrived here August 16th, and one man died of yellow fever August 5th. This vessel lay in the stream seven days, where her cargo of coffee and sugar was put in lighters and sent to the city; but the tallow in hides and casks, which was taken on board at Buenos Ayres, was retained for ballast until she was hauled to the wharf, where it was landed previous to the gale. She was ventilated and white-washed while in the stream, except that part of the hold which was occupied by the tallow.

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* The ship Ann-Maria had been used as a transport to convey Devereux's troops from Ireland to the West Indies; afterwards she proceeded to New-Orleans, from whence she sailed to Havre de Grace, with a cargo of cotton; then returned to New-Orleans, where another cargo of cotton was put on board of her, with which she sailed to Pensacola, and there it was delivered on board of a French ship lying in the bay. The Ann-Maria then sailed back again to New-Orleans, where she was purchased by her commander, Capt. Anthony R. Gale, (in May 1821,) who being anxious to know what was the condition of her ballast, and how much of it was iron, employed about twenty men for a week to overhaul it, and to clean out her limbers. None of these men were taken sick, and the chief mate, who superintended the work, enjoyed uninterrupted health from that time until she was brought to this city. This vessel sailed from New-Orleans in ballast, and received no cargo on board from the time she last left that port, until she arrived here. It appears, therefore, that none of the alleged causes of yellow fever existed on board of her previous to her arrival at the sickly port of Havana, where she must have received the contagion of yellow fever, which manifested itself on her passage to this port, and which was communicated to six persons employed on board in discharging her ballast at our Quarantine, in September last.

The crews and passengers of those three ships, forty-five in number, were released from Quarantine before the gale, as twenty days had expired since the last death had occurred on board; but three of the crew remained in each vessel to take care of them.

The ship Le Herondelle having arrived in distress, permission was given to take her to the United States wharf, the 28th of August, three days after she came into port. Two of her seamen died at Havana, one on the 18th, the other on the 20th of June; but from that period until the 1st of October, when she was taken from Quarantine, not one of her crew, consisting of eleven persons, was taken sick, although the ship lay alongside of the United States wharf the whole time, with all of them on board, except the supercargo. Her cargo consisted of sugar, cotton, rum, indigo, and dye-wood, which was principally discharged before the gale; and what remained in her was but little damaged.

The brig Lion arrived in ballast at this port, August 14th, from a four months' cruise off the coast of Spain, via St. Bartholomews and St. Eustatia, where her crew, of one hundred and fifty men, were discharged in health. The crew which navigated her to this place, were shipped from those islands without going into port with the vessel, and continued in health during the passage. This brig, after performing the usual Quarantine of four days, directed for vessels coming from healthy West India ports, with healthy crews, was permitted to proceed to the stream opposite the city, but three days after, she was removed to Quarantine again, as one of her seamen was taken sick with fever of a suspicious character, which afterwards, however, was ascertained not to be the yellow fever.

The schooner Betsy from Wilmington, N. C. was taken to the wharf the 27th of August, the day after she arrived, where her cargo of cotton and turpentine was unloaded in three days. Her crew consisted of five persons, who were healthy during the voyage, and continued so when discharged, the 5th or 6th of September.

From the foregoing statement, it appears that three of those vessels, lying at the wharf before the storm, arrived in ballast; two others had discharged their cargoes in good order; and that part of the cargo which was unloaded before the storm from the sixth vessel, was also in a sound condition. The crews of the three first-mentioned ships had been discharged, except their ship keepers; and the ship Le Herondelle and brig Lion being foreign vessels, their crews, who were also foreigners, and used to the West India climate, continued attached to their respective vessels until late in October, and enjoyed good health all the time they were at Quarantine, although they resided at the United States wharf the whole time. The gale drove the ship Ann-Maria on the rocks, 630 feet from the United States wharf, where she remained until the 20th of October.* The ship Lucy-Ann was driven on shore, but was got off the same high tide, and again hauled to the wharf. The ships Amphion and Le Herondelle retained their situations at the wharf. The brig Lion and schooner Betsy were sunk at the wharf, where the brig remained until November, and the schooner about three weeks.

All the persons who were taken sick with yellow fever the last year at Staten-Island, after the 3d of September, had been repeatedly on the United States wharf, near to those vessels, for some hours at a time, or worked on board one of the four ships from Havana, except Capt. Van Beuren's daughters, and the three females of the Sinclair family. This wharf having been the focus of the disease, I have deemed it necessary to relate minutely the circumstances of some of those vessels to the time of the gale, and of others until discharged from Quarantine, but shall again have occasion to refer to some of them when tracing the probable source of disease, in individual cases.

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* If the yellow fever was produced by the cargoes of vessels getting damaged in the gale, another cause must be assigned for its taking place in this ship, as she arrived without any cargo, and was at too great a distance to be in the least affected by any other vessel.

A great many vessels having been driven on shore the 3d of of September, the condition of those only which were stranded between the United States and the State wharves, will be examined into; for the others were at such a distance, that they could have no agency in adding to the infected atmosphere of the place. Fourteen vessels were driven on shore within the above mentioned limits, and they can with evident advantage be divided into two classes, viz: those that were from healthy ports, and those which sailed from unhealthy ports, or had sickness on board. The vessels constituting the first class, are the ship Belle, and brig Belvedere, from Savannah; the brigs Neptune's Barge, and South Carolina, and the schooner William and Joseph, from Charleston; and the schooner Pacification, from Jacquemel. Some of them arrived during the gale, and would have been permitted to proceed to the city immediately after their examination: others had been released from Quarantine, but they were prevented from getting under weigh by the violence of the storm. The crews and passengers of those vessels, consisting of sixty-four persons, were in perfect health during the passage, and when discharged from Quarantine. The cargoes were principally cotton, part of which was wet, but it was all sent to the usual places of deposite as fast as lighters could be procured to take it away. No sickness was occasioned by them or their cargoes, as many persons from the city who were employed to get those vessels off the shore enjoyed uninterrupted health.

The second class has been divided into those vessels which were driven alongside, or near to the United States wharf, and those at the greatest distance from it; that the disease which occurred in the Sinclair family may be accounted for from another source than the infected vessels. In the first subdivision is the brig Andreas, from Marseilles, via Cape Haytien, with a cargo of wine. She arrived at Quarantine August the 15th, in nineteen days from the last-mentioned port. She had on board five passengers, all well; her crew, of ten persons, were shipped in Sweden, and were healthy during the voyage. On the 22d of August, her carpenter, who went to bed well, died in the night: the appearances on dissection clearly indicated that his death was occasioned by apoplexy. The same day one of his shipmates was taken sick with slight fever; and on the 27th of the same month two more of the crew were taken ill with fever of a suspicious character, accompanied with great tenderness of the abdomen, which was attributed to their provisions being cooked in copper utensils that had their tin coating worn off by long use. They all recovered, and two of them returned to the brig the 2d of September. After discharging part of her cargo which was not injured, she was got off, and hauled to the United States wharf, where she was unloaded and purified, but remained there until the last of September, when she was anchored again in the stream. While she was at the wharf, between the 14th and 26th of September, the captain and two seamen sickened with yellow fever, one of whom died, and two recovered. The sloop Spark arrived August 29th, in eighteen days from Havana, with a crew of five persons, and two Spanish gentlemen, passengers. They were in health when they arrived here, and when they left Quarantine. The former captain died at Havana, August 7th, and a seaman on the passage, August 13th, both of yellow fever. This vessel was sunk at the wharf, with all her cargo, consisting of sugar, which was totally lost; for it had been under water three days before any attempt was made to save it; and she remained under water until she was removed to Long-Island to be repaired. The brig Mentor arrived September 1st, in sixteen days passage from Matanzas, and was navigated by seven persons, who went from, and returned to, New-York in her. They and the passengers enjoyed perfect health during the voyage, and were discharged from Quarantine the 6th of September. The place she sailed from was represented to be healthier than usual at that season of the year, but it was considered by us a sickly port. Her cargo of sugar, coffee, molasses. and fruit, was damaged, and sent to the city immediately and sold. The brig Nancy arrived August the 31st; she had twenty-eight days passage from Port-au-Prince, via Rum Key. At the latter place she was loaded with salt, which was all lost when she was driven on shore. The same crew, consisting of seven persons, sailed in her from Charleston, and went the voyage round. None of them had been sick since they shipped in her; and they were in health when discharged from Quarantine, September 5th. She had one passenger, who was also well. The brig Albert arrived September 2d, after twenty-seven days' passage from Point Petre, Guadaloupe. She was navigated by nine seamen, who sailed the voyage in her from Portland, without any of them being seriously indisposed; and during the forty days they remained at Point Petre, they did not hear of the death of any person belonging to the shipping in that port. This vessel was driven to the United States wharf, where her cargo of molasses, wine, and oil, was discharged in good order. She was soon after hauled into the stream, and her crew continued healthy. The schooner Native, after twenty days' passage from St. Thomas, arrived the same day as the brig Albert, and was driven to the wharf near to her, where her cargo of rum, wine, sugar, and hides, (which last article, being on top of the others, was quite dry,) was discharged into lighters, very little injured. She was removed to the south side of Staten-Island, about the 14th of September, to be repaired. Her crew consisted of eight men, six of whom went in her from New-York, and two were shipped at St. Thomas, where one of the original crew died of fever, after ten days' illness. The only passenger who came in her, was a gentleman who had resided many years in the West-Indies, and was recommended to come to the United States to obtain relief for a chronic affection of his liver, of which disease he died the day of his arrival. No other person than the two above mentioned had been sick during the voyage, except a man with rheumatism. Three days after she was removed from Quarantine, one of her seamen was taken ill with yellow fever, September the 17th, and died on the 23d of the same month. Of those six vessels contained in the first subdivision, it appears, that the cargo of the brig Mentor is the only one materially damaged, (and it was sent away in lighters as soon as it could be discharged,) except that of the sloop Spark, which was under water three days. When the upper boxes of sugar were taken on the wharf, some of the contents, which had not been washed out by the water, ran out on the wharf and produced an unpleasant smell; but this could have no influence in producing the fever, as it had already commenced. The remaining two vessels, whose circumstances are to be described in the second subdivision, were 625 feet from the United States wharf, and at least 450 feet distant from those vessels mentioned in the first subdivision. The brig Andromache, the nearest of the two to the United States wharf, arrived the 26th of August, in twenty-one days from St. Thomas, navigated by eight persons, who sailed from, and returned to, New York in her. She had also one passenger on board. On her passage to this port one of her seamen died of fever, August the 10th, after nine days' illness; the captain and one sailor were sick with a similar disease at the same time, and recovered; all the other persons were healthy during the voyage. Six or seven men came from the city, and worked at this vessel, with three of the crew, for four weeks, during which time they occasionally slept on board, but not one of them was indisposed. Her cargo of rum, sugar, and indigo, was all discharged in lighters, while she remained in the stream, except eight hogsheads of sugar retained for ballast. This was damaged when she was driven on shore; previous to which event she had been ventilated, and partly white-washed. The brig Copernican arrived September 1st, after a passage of twenty days, from St. Jago de Cuba. She was navigated by eight persons, who all went from New-York in her, and enjoyed perfect health during the voyage. All of them, except the mate and cook, were discharged from Quarantine about the 8th of September, at which time the mate's wife and child came from the city, and remained on board until the 28th of September. They and the labourers from the city, who were employed to get her off the shore, which was not effected until the 26th of the same month, were in health the whole time. Her cargo of sugar, which was half damaged, was carted to the steamboat wharf, and from thence sent to the city.

The particular circumstances and situation of all the vessels that could possibly have any agency in producing the pestilence at the Quarantine ground, the last season, have now been accurately detailed. In order to show that the contagion of yellow fever, from infected vessels, did not extend its baneful effects a great distance in the pure air of the country, and to offer an explanation of some facts, which occurred in the Sinclair family, it will next be necessary to give an account of the number and situation of dwellings that are near to the river, and are contained in the space of 1400 feet along the shore, where the above-mentioned vessels were stranded, to state their distance from the United States wharf, which may be considered as the centre around which the infected vessels lay, and to specify the distance of those dwellings from some particular vessels. Five dwelling houses and the hospital are contained within the above-mentioned limits, viz. Capt. Arnet's and Mr. Fountain's, situated on the north of the United States wharf, the first 280 feet, and the second 510 feet from that part of it which was occupied by the four ships from Havana; Capt. William Van Beuren's 290, the Health Officer's 570, Mr. Vanderbilt's 810, and the nearest part of the hospital 940 feet distant from the same wharf, on the south side. The brig Mentor was stranded 100 feet from Capt. Van Beuren's house. The distance from the place where the brig Andromache laid to Mr. Vanderbilt's house, is 225, to the Health Officer's 300, and to the hospital 350 feet. From the place where the brig Copernican was driven on shore, to

Mr. Vanderbilt's house, is 150, and to the hospital 275 feet. These vessels were nearer than any others to the buildings above mentioned. After the ship Ann-Maria was driven on the rocks, where she lay upwards of six weeks, she was only 250 feet from Mr. Fountain's house, but 630 feet from the United States wharf. All those buildings are 300 feet from each other, except Mr. Vanderbilt's house, which is 160 feet from the hospital. The land about them is under cultivation; in fact, it is a country place, and its healthiness has never been questioned. The shore is bold and rocky, the ground which gradually rises from it, lies open to the ocean, and is cooled by its summer breezes. There are within the Quarantine limits only two wharves, which are built of sound materials, and covered with wholesome earth. The only stagnant water and marshy land in this part of the island, are situated about two or three miles from the Lazaretto, and they are of very limited extent. But if they were much nearer, no injurious effect would result from them, as the only wind that would waft their miasmata to the hospital, is broken off by the intervening high land.

The foreign and local circumstances, that could have any influence in causing the fatal fever which prevailed at the Quarantine ground, having been fully detailed, I shall next attempt to trace the source of this disease, by its baneful operation on individuals. Although the particular change which this species of contagion produces in the atmosphere, like that from small-pox, measles, plague, &c. cannot be defined; nevertheless, we may avoid such sources of it as experience has proved to be dangerous, and not place our whole security in guarding against the foul air arising from animal and vegetable decomposition alone; for in manufactories where those putrid matters abound, and the stench occasioned by them is almost insupportable, the health of the workmen and the inhabitants in the vicinity, is infinitely less injured than we should expect to find it, from the powerful impression which such corrupt materials make on our senses.

A few persons were admitted into the Marine hospital, with yellow fever, in the months of July and August, from vessels at Quarantine, lying in the stream; but the first person taken sick with that disease, after a few vessels were hauled to the United States wharf, and subsequent to the gale, was Seth Waite, a lad seventeen years of age, who had performed the voyage in the ship Lucy-Ann, last from Havana. This vessel was much injured by the storm, and required repairs. This circumstance induced the Board of Health, on the sixth of September, (before the appearance of any malignant disease among the shipping which were stranded at Staten Island,) to grant permission for her removal to the ship yards, in the upper part of the city, as she had been previously purified. After this vessel left the Quarantine ground, and as she was proceeding to the city, this young man was taken ill, September 6th, and he died in the Lazaretto the 11th of the same month. A seaman died of yellow fever, August 13th, on board of this ship, and the mate, who was the last person taken sick, was convalescent and walking the deck the 20th of August, the day she arrived. This vessel, coming from an unhealthy port, having lost one of her crew with the yellow fever, and the mate having been sick with the same disease, on the passage, were sufficient to excite our suspicion that she was an infected vessel; although she had only stone ballast on board, from the time that she sailed from New-Orleans, until she arrived at this port, via Pensacola and Havana, and her hatches had been off the whole voyage. It has been remarked, that her ballast was all overhauled, and every part of the hold and forecastle was ventilated and white-washed; between decks were scrubbed with an alkaline solution, and afterwards varnished. This was nine or ten days before Waite was taken ill. This young man was actively employed for several hours, in the afternoon and night of the 3d of September, during which time his clothes were drenched with rain. It was three days after this occurrence he was taken ill, which was eighteen days subsequent to any sickness on board of his vessel. Was this disease lurking in his system for two or three weeks? Could it have been caused by the violent motion of his vessel, disengaging a greater quantity of foul air during the storm, or by the vitiated air from the ship Ann-Maria alone, or combining with that from his own vessel, soon after she was hauled alongside of the Ann-Maria, and thereby rendering the atmosphere about them more loaded with contagion than either vessel could alone produce? In answer to these queries it may be said, that the disease would have appeared immediately after his great fatigue and exposure in the gale, if it had been in his system eighteen or twenty days. If it arose from an increased quantity of infected air disengaged during the storm by the great motion of his vessel, it probably would not have commenced as early as the third day after the gale, for not one of the cases which occurred took place in less than five days after the first exposure to infected vessels. I am, therefore, of opinion, that this disease was caused by the contagion from the ship Ann-Maria, in addition to that of his own vessel, for his ship was hauled along side of her the 31st of August, and he had to pass over her deck to get to the wharf. Six days after this date he was taken ill, which period of five and six days will appear by the majority of the cases subsequently stated, to be the usual time that this disease commences after its contagion has been received into the system.

The Rev. Mr. Hardenbergh having engaged to officiate in the church near the Quarantine ground, on Sunday, September 2d, visited his friend, Captain William Van Beuren, the preceding Saturday afternoon, when he amused himself in fishing about two hours, on the United States wharf, near to the bows of the ships Ann-Maria and Lucy-Ann. The ship Le Herondelle's cargo was discharging at the same time on the opposite side of the wharf, and this vessel was about two hundred feet from Captain Van Beuren's house, where Mr. Hardenbergh slept that night. He returned to the city on Sunday afternoon. The next Friday, September 7th, six days after his exposure to the contagion of the above-named ships, he was taken sick, and died the 12th. If his disease was yellow fever, (which appears to be the general belief,) he without doubt received the infection while on or near to the United States wharf.

Jack Brittan, a coloured man, worked on the United States wharf, at the cargo of the ship L'Hirondelle and other vessels, from Monday, September 3d, before the gale commenced, to Saturday noon, the 8th, at which time he complained that he was unwell. He went home, and died the 14th of September. In this case the disease began in five days after he was first employed at the United States wharf.

Miss Sarah-Ann Van Beuren, aged 11 years, was on the United States wharf, Tuesday, September 4th, and near to the brig Mentor, which was driven on shore before her father's house. Five days after, she was taken sick, viz. Sunday, the 9th, and died the 13th of September.

John Hawthorn, who was on a visit to his father, at Staten-Island, was employed several hours on Tuesday, September 4th, in pumping out the ship Amphion, lying at the wharf. He was taken ill on Sunday morning, the 9th. and died the 13th of September. Here we also observe the period of five days intervening between his going on board the Amphion, and the time he was taken sick.

Peter Andrews, a seaman who belonged to the ship Lucy-Ann, was taken ill the 9th of September, five days after his great fatigue in the storm which raged in the afternoon and night of the 3d, at which time he would be more predisposed than before to be infected with the contagious air which was probably discharged from its confinement, by the concussion of the vessel against the wharf and shore. He died the 14th of September.

Edward Wilson, John Young, and James Williams, with eight other men, were sent on board the ship Ann-Maria, Wednesday afternoon, September 5th. They were all engaged in discharging her ballast until Saturday afternoon, the 8th. On Monday, the 10th, they returned to Staten-Island for employment, but they were taken sick in the afternoon, and sheltered themselves in Captain Arnet's hay-loft, until the next day, when we were informed of their illness, and had them brought to the hospital; where the two first named men died, September 13th, and the last on the 14th. In these three cases, we perceive the remarkable coincidence of those men becoming ill at the same time, and that in five days after they were first exposed to the contagious air of the ship Ann-Maria.

John Patterson first came to the Quarantine ground, for employment, the 5th or 6th of September, and worked on board of the brig Nancy, from Havana, and some other vessels. On the 11th of September he was taken sick, and died the 21st of that month. Here again we have the same period of five or six days after this man was first exposed to contagious air, before he was taken ill.

A girl, about thirteen years old, who carried refreshments to the dock builders, several times a day, while they were working on the United States wharf, was taken ill September 11th, and died September 16th. This is a period of seven days after the gale. But the wharf was so much injured by the storm, that the men discontinued their work, until a new contract was made to finish the work. It is probable, therefore, that this girl did not go there until a day or two after the storm.

Christopher Chergold, three or four days after the gale, went on board of several vessels that were stranded, and also on the United States wharf. Five or six days after he first went on board of those vessels, he became sick; he was put under mercurial treatment, and recovered.

Perry Roads, one of the dock builders, who was employed either three or four days after the storm, to discharge or work at the cargo of the ship L'Hirondelle, was taken sick the 12th of September, and died the 23d. In this case, we notice the same period of five or six days after he had been working, either on board, or alongside, of the ship L'Hirondelle. This man and John Patterson had apparently struggled through the disease, and they were considered convalescent from the sixth to the ninth days of their illness, as they were during this time free from fever, their appetite returned, and they slept well; but all these favourable symptoms were unexpectedly changed on the ninth day, when Patterson, who bled freely from the gums in the early stage of the disease, had a more profuse discharge of blood from them, after it had been suspended for a day or two; he also evacuated from his stomach and bowels large quantities of dark blood, he became comatose at the same time, and continued so until he died. Perry Roads, on the ninth day, lost his relish for food, and although he took his drinks and medicines freely, still he became hourly weaker, and had no pulse at his wrist twenty-four hours before his death; yet his understanding was unimpaired to the last.

John White, Quarantine gate-keeper, was frequently on the public wharf in the performance of his duty; he slept one night on board of the brig Mentor, was taken sick the 13th, and died September 18th.

Another daughter of Capt. Van Beuren's, who attended on her sister particularly during the first part of her illness, was attacked with fever the 14th of September, five days after she first nursed her sister. This young lady recovered after a severe and tedious illness.

Capt. Slaight, who on the 6th of September was first employed in transporting the cargoes of vessels from the public wharf to Brooklyn, took in a second load on Saturday, the 8th, and slept that night on board of his lighter, which was lying at the United States wharf. He mentioned to his relations, when sick, that the last bale of cotton which he put on board his boat, was so offensive as to produce excessive vomiting, and he was impressed with the belief that he caught the fever from it. But I am of opinion, that the languor which consequently ensued after vomiting, and his apprehension of danger from the cotton, predisposed him to be infected by the contagious atmosphere, which, at that time, prevailed about the wharf, and in which he slept the same night. He was taken sick the 14th of September, in the morning, five days after he slept at the public wharf, and died on the 19th.

Capt. William Arnet, an Inspector of the Custom-house department, was informed, on Tuesday afternoon, September 11th, that three men were sick in his hay loft, near to his house:

as he was alarmed for the safety of his family, he went there to have them removed, and the same evening, at Dr. Harrison's, he expressed his apprehension, that he might have taken the fever from those men; and he was impressed with the belief that he would get safely through the season, provided he should escape that exposure. Early the next day he removed his family to Elizabethtown, and on Sunday morning, September 16th, when there on a visit, he was taken sick. This was precisely five days after he was in the hay loft with the sick men, and he died the 20th of that month.

While the brig Andreas was lying at the United States wharf, her Captain, Andrew Ronning, and Michael Dolman, one of her seamen, were taken ill on the 17th of September; and James Watkins, a seaman who belonged to the schooner Native, then at the south side of Staten-Island, also sickened the same day. The first recovered, and the two last died on the 23d of September.

Francis Morgan and Peter Howard went to work on board of the ship Ann-Maria, the 12th of September, with sixteen other men, and continued there until the afternoon of the 18th, the same evening they began to complain. This was six days after they were first exposed to the contagion of this vessel: they both recovered.

Capt. Van Beuren was taken ill the 23d of September, and recovered. Magnus Landbour, a seaman who belonged to the brig Andreas, sickened on the 26th of September, and recovered.

Ephraim Tompkins, the last person who was taken sick with yellow fever at the Quarantine ground, went on board the ship Ann-Maria, with seven other men, on the 2d of October. He became ill on the 7th, five days after he was first employed in this vessel, and he died on the 11th.

It cannot escape observation, that all those persons whose cases have now been stated, were either on board or alongside of infected vessels, (except Captain Van Beuren's daughter, who was taken ill the day after her sister was buried,) and to the contagion of those vessels their sickness must be attributed. But what is equally worthy of notice, and not less important, is the remarkable circumstance, that twelve of those twenty-five persons were taken sick on the fifth and sixth days after they were first exposed to infected vessels, viz: The Rev. Mr. Hardenbergh, Jack Brittan, Miss Sarah-Ann Van Beuren, John Hawthorn, Edward Wilson, John Young, James Williams, John Patterson, Christopher Chergold, Francis Morgan, and Peter Howard. From this important fact, we have been led to fix on the time when, and to offer our reasons why, this disease occurred at the precise period of five and six days, in seven more of the twenty-five cases, viz: Seth Waite, Peter Andrews, the girl who attended on the dock builders, Perry Roads, Miss Mary Van Beuren, Capt. Slaight, and Capt. Arnet, who, although they had been longer than six days exposed to an infected atmosphere, nevertheless escaped the disease until they were exposed to a more concentrated contagion, or they had, at such special exposure, a greater predisposition to take the fever than previously existed in their systems. The remaining six persons, viz: John White, gate-keeper, was taken ill twenty days after the infected vessels were brought to the wharf, and nine days after the brig Mentor was driven on shore, very near to the place where he was stationed to keep the gate; but when he slept on board the brig Mentor cannot be ascertained. Capt. Ronning, Michael Dolman, and James Watkins, sickened thirteen days after their vessels were driven to the wharf. Magnus Landbour was taken ill twenty-two days after the same period: and Capt. Van Beuren, one month after the infected vessels were hauled to the United States wharf, and nineteen days after the gale. If the inquiry had been made, it is probable that some special circumstances might have been discovered in each of these six cases, five or six days previous to their illness, as occurred in the seven cases before stated. But our attention was not drawn to the fact, that so many of those persons who had the yellow fever, were taken sick at this particular period after their exposure to the cause of it, until the pestilence had ceased, and they were discharged, or had died. It may be well to remark, that Capt. Van Beuren removed with his family on Saturday, September the 15th, from the Quarantine ground to a place where he was breathing a pure atmosphere. This would make him more liable to take the disease when he opened the stores on Monday following, as the infected air in them would probably be rendered more malignant by their being shut on Saturday. He was taken sick six days after he opened the stores, and subsequent to his removal from his own house.

The Sinclair family, who remain to be noticed, have not been enumerated in the order they sickened, with the other persons who had the yellow fever, because all those persons had been previously exposed to infected vessels; and although Captain William Arnet and Miss Mary Van Beuren appear to have derived their disease from personal contagion, yet the evidence may not be considered conclusive. It has been stated, that the Sinclairs resided in the north basement story of the hospital, 940 feet from the United States wharf, and 275 feet from the brig Copernican, the nearest vessel to their abode. She was from St. Jago de Cuba, and her crew had been healthy the whole voyage. After she was driven on shore, the mate's wife and child lived on board, with impunity, twenty days, during which time the damaged cargo was discharged. Mr. Vanderbilt's family, consisting of ten persons, lived 125 feet nearer to this vessel than the Sinclairs did, and the same wind that would blow the foul air of this vessel upon the dwelling of the latter, would also carry it to the former; nevertheless, Mr. Vanderbilt's family all enjoyed good health. The brig Andromache was also 125 feet nearer to Vanderbilt's, and 50 feet nearer to my residence, than to Sinclair's. Hence, it may be inferred, that they did not take the disease from those two vessels, or all those persons from the city who resided on board, and the two families before mentioned, whose members were more than three times as numerous as the Sinclair family, could not have escaped the disease. As we do not know how far contagious air may be conveyed by a current of wind, and produce its deleterious effects, it may be contended, that the Sinclairs derived their sickness from the infected air of other vessels, although at a greater distance from their residence than those two brigs. I would reply, that no other vessels suspected to be infected, but the two brigs, were nearer to their residence than 750 feet, and as four out of six families whose houses are within the limits before described, lived twice as near to those vessels as the Sinclairs, and continued healthy, with the exception of Capt. Arnet, their disease cannot be traced to that source. In further corroboration of the inference that the Sinclairs did not derive their disease from the stranded vessels, and as a proof that the contagious air on board of vessels is not communicated 250 feet in a pure air, aided by a favourable current of wind, the following fact concerning the ship Ann-Maria may be adduced. This vessel was indisputably an infected one, for the men who worked on board of her and were taken sick, could not have derived their disease from any other source, as she lay 630 feet from any other vessel, and the same distance from the United States wharf. Now this vessel remained six weeks only 250 feet from Mr. Fountain's house, during which time the wind frequently blew in a direction from her towards his house, yet none of his numerous family or boarders were taken sick. It is reasonable to suppose, therefore, that if the Sinclairs remained at home, they did not receive the disease from any vessel, as the stranded vessel nearest to their residence, (and she was not an infected one,) lay 25 feet farther off from it than the ship Ann-Maria lay from Mr. Fountain's. Both of Sinclair's daughters assured me, that they had not been nearer to the United States wharf, or to any of the vessels, than their dwelling, for some weeks before the gale; and they and their father informed me, that Mrs. S. never was nearer to any of the vessels than her residence, except in going to and from the Health Officer's house twice, she passed as near as 100 feet to the brigs Copernican and Andromache, and about 300 feet from the other suspected vessels. James Sinclair, health office boatman, had been on the United States wharf, and among the infected vessels, but not after the 11th of September, and from that time until he was taken sick, he was not more exposed to the infection of yellow fever than he had been every season before, for nineteen years past; and as he was not taken sick before the 28th of that month, it is probable that he received the cause of his disease from another source.

Having fully examined into the situation of all vessels which could have any influence in causing the sickness which prevailed in the Sinclair family, and finding none to which we can ascribe it, we shall now endeavour to account for their disease from personal contagion and foul clothing. Mr. and Mrs. S. visited Hawthorn during the latter part of his illness; and the 13th of September they and their daughters were at his house making arrangements for his funeral. Mrs. S. did not visit any other person who had yellow fever, besides Hawthorn; but on Monday, the 17th of September, she received the bedding and clothing of Wilson, Williams, Young, and Waite, who died of yellow fever on the 11th, 13th, and 14th of the same month, some of which, particularly Waite's, were so filthy, that she directed her daughters to go out of the wash-house, while she carried the clothing through it to the yard, where she put them into cold water. On the 22d, early in the morning, five days after she had this foul clothing in her arms, she was taken sick, and died on the 26th of September. Her daughter Jane, from the commencement of her mother's illness, so frequently laid her head on her mother's bed and kissed her, that her father twice took her away, as he was apprehensive that she might contract her mother's disease. She was taken sick, September 27th, five days after her mother first sickened, and she died October the 4th, with black vomit, previous to which time she had been three days under a free salivation. James Sinclair was in bed with his wife when her fever commenced, and he was taken sick the 28th of September, six days after he was exposed to her contagious fever. He died the 3d of October. Sally Sinclair slept with her sister the same night that Jane was taken ill, and five days afterwards, on the 2d of October, she was taken sick, but recovered. Could those four persons have received the infection from one and the same source, viz. from Hawthorn? I am of opinion that they did not take the disease from him, and the daughters did not go there until his death. They sickened at very different periods, after they were at Hawthorn's; but if they had caught their fever from him, we should expect that some, if not all of them, would have been taken ill in five or six days after being at his house, and within a few hours of each other, as occurred in the cases of Wilson, Young, and Williams, who went on board the ship Ann-Maria together, and sickened in an hour or two of each other. The same circumstance happened in the cases of Morgan and Howard. But Mrs. S was not taken sick until the lapse of eleven days, and the daughter who last sickened, not until twenty-one days after they visited Hawthorn. These reasons are conclusive, in my opinion, that the Sinclairs did not contract their malignant fever at Hawthorn's; but, that Mrs. S. caught her's from the foul clothing she had in arms, her husband and daughter from her, and Sally S. from her sister Jane. The three last were not exposed to the infected clothing, consequently they must have taken the fever from personal contagion. I am led to this conclusion from the important fact, that three of them became ill in five, and the other in six days after being exposed to the special exciting causes before related; for their becoming sick at that particular period cannot be considered as the effect of mere accident, otherwise it would not have occurred in nineteen persons out of twenty-five, who derived their disease from the infected vessels at the Quarantine ground.

From the foregoing faithful and minute statement of facts appertaining to, and connected with, the fatal fever which prevailed at the Quarantine ground last fall, are drawn:

First, *That the yellow fever was not generated from local causes, inasmuch as that part of Staten-Island where the Lazaretto is situated, is as healthy as any place in the United States, and the contagion of that disease never before prevailed among the inhabitants.* No local impurities having existed there that could generate a malignant fever, some persons might attribute the pestilence to the cargoes which were damaged in the memorable September gale: but as those cargoes were so recently injured, and would have been totally lost if retained only a short time at Quarantine, they were permitted to be taken away before they were in such a state as to generate foul air; and as a proof that the fever was not produced from this cause, I refer to the circumstances of particular vessels herein stated, from which it will appear that those vessels whose cargoes were most injured, were not rendered more unhealthy in consequence of such injury. On the contrary, those vessels which arrived in stone ballast, with their hatches off all the passage, from very sickly ports, as Havana, were the most fatal to the persons who remained on board.

Many vessels were driven so far ashore, that at low tide they were entirely out of water, and they were but partially in it at high water. Some days after the gale, the worms and shell-fish that adhered to their bottoms, died and became putrid, which rendered those vessels offensive, and to this cause, perhaps, some may attribute the fever. Those small worms being imbedded in the water-soaked bottoms of vessels in their natural situation, and the shell-fish adhering by suction, would not die in less than four or five days, while undisturbed in their nidus or attachments; and admitting that they became putrid immediately after their death, still a few days must necessarily elapse, before any injurious effects would ensue from them. Therefore, a malignant fever, from this cause, could not take place in less than ten or twelve days after the vessels were driven on shore; but the yellow fever had actually commenced in six cases, seven and eight days before this period, and even before the offensive smell from those vessels was noticed. That the fever did not arise from that cause, is further confirmed by the fact, that in those places where the air was rendered most impure from those insects, no person was taken sick, except Mr. James Ward, who had remittent fever. While at the United States wharf, where the principal cause of the pestilence existed, none of the vessels which lay at or near to that wharf, were in the least offensive, from putrid worms, or shell-fish, as the tide never left them dry.

It is evident, that the yellow fever was an imported disease, which existed in those vessels at the wharf previous to the gale, but was no doubt greatly increased by it, as it brought many more infected vessels near to the wharf, and consequently augmented the quantity of infected air at that place.

Secondly. *I am of opinion, that the yellow fever is contagious, in the same manner as jail, hospital, and ship fever, and is communicated from one person to another, through the medium of the atmosphere, tainted with the specific matter of this disease, and particularly in the impure atmosphere which exists in low, confined, and crowded parts of cities, during the warm months of the year.* From its contagious character alone can the sickness which took place in the Sinclair family, be satisfactorily accounted for: as they had not been exposed to any other source of disease than personal contagion, (and foul clothing in the case of Mrs. S. only.)* That part of the family who escaped the fever, were two adults and two children, and they lived and slept in a building 80 feet from the hospital, near to the wash-house: they were often in the rooms with their sick relatives for a short time. From this circumstance, as well as many similar ones, it would appear that a continued exposure of some hours to the air of an infected room, or a very near approach to the sick, and repeatedly inhaling their contagious exhalations, in addition to a predisposition, is generally essential to contract an infectious fever. It is observed that few or none of those persons who respire infected air for a few minutes are endangered by it. This circumstance will explain why so many persons escaped the disease, whose curiosity led them to

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* When clothing is tainted with the specific matter of small-pox or measles, those diseases will be communicated by such fomes, and they are admitted to be propagated by contagion, when taken in this manner, because the specific matter is not so altered as to produce any other disease. Now, Mrs. Sinclair, having had the same disease, (viz. yellow fever,) as Waite, and others, whose clothing she washed, it must be allowed, on the same principle, that her disease was also produced by the specific contagion of yellow fever.

Staten-Island to view the wrecks of the great number of vessels driven on shore at and near to the Quarantine ground. That the constitution frequently adapts itself to a contagious atmosphere, and resists its influence, the memorable fact which occurred in the year 1819, will clearly illustrate. At that time nearly two hundred inhabitants, who had been living a fortnight in the infected district of Old Slip, were furnished with accommodations at Fort Richmond, on Staten-Island. After their removal, eight or ten persons only were taken sick with yellow fever. Now, as non-contagionists admit that the yellow fever is taken in such an infected atmosphere, can the escape of so large a proportion as one in twenty be accounted for on the principle of infection? With what propriety, then, can they deny its contagious character, merely on account of its being but seldom communicated from one person to another in clean and well ventilated apartments, and especially as fevers, acknowledged by themselves to be infectious, are not more frequently contracted than yellow fever, where similar precautions are used? Surely we have much greater reason to ask them to show how any person can escape the infection, after having lived in it a fortnight, than they have to doubt the contagious character of the same disease, because so many escape the contagion from the sick, whose apartments can very seldom be contaminated, where due attention has been paid to cleanliness and ventilation. This objection against the contagious character of yellow fever, proves too much, and must therefore be rejected, for it also proves that it is not even an infectious disease.

Because the yellow fever is not as contagious as small-pox, measles, or plague, is it therefore to be considered as non-contagious? Are those diseases equally contagious? Is each of them equally contagious at all times, and at all seasons of the year? Are they uniformly as malignant or fatal every time they occur? Facts give a ready answer to these questions, that there is a wide difference among them, not only in the degree of their contagious property, but even in the active contagious character of each disease at different times and seasons of the year. What is the standard to which we must appeal to know when a disease is to be termed contagious or non-contagious? There is so great a variation in this respect, among those diseases admitted to be contagious, and the same disease at different times, that there does not appear to be any definite rule laid down to govern us. The best practical one would be to consider those diseases as certainly contagious or infectious, which are capable of producing a similar disease, whether it is communicated to every person who may be exposed to the contagion, or only to one person in a hundred. When a disease, on this principle, is generally admitted to be contagious or infectious, by carefully observing the circumstances in which it is more or less frequently communicated, we shall in time be enabled to judge to what degree it is so, to know the risk incurred by exposure to it, to take suitable measures for its extinguishment, and provide comfortably for the sick, with the least hazard to their friends and attendants.

Thirdly. *I am of opinion that the yellow fever is produced from a specific contagion; for the twenty-nine persons, who sickened in consequence of their intercourse with the Quarantine ground, all contracted the same disease,* twenty-one of whom died, and fourteen of that number had that fatal symptom, black vomit. If this disease is only a higher grade of bilious fever, and is not produced by a specific contagion, but from marsh miasmata, or animal and vegetable putrefaction, as inculcated by our professional opponents on this subject, would not some of the diseases which ensue from those causes, such as bilious remittents, remittents, and intermittents, have also appeared at Staten-Island, from the same cause which occasioned the yellow fever? No idiopathic febrile complaint existed among the inhabitants, in the neighbourhood of the Quarantine ground, previous to the gale.* If, therefore, it is a fact that yellow and bilious fevers originate from one and the same cause, a fairer test could not be conceived to show their identity, than what actually existed at that time at Staten-Island. If either of those diseases had prevailed there before the gale, followed by the other soon after it, doubts might have arisen, whether both fevers were not derived from the same origin. For instance, if bilious fever had prevailed at Staten-Island, before the 3d of September, and the yellow fever had ensued immediately after the gale, (as actually happened,) and attacked some of the inhabitants who had been previously exposed to the miasmata which gave origin to bilious fever, but who had also been exposed to the contagion of yellow fever, by going on the United States wharf, or on board of infected vessels, our medical opponents would not have allowed that the disease was derived from the infected place or vessels, but attribute it to the same source which gave rise to the prevailing bilious fever, and consider such a case as a proof of the identity of the cause of both diseases, and call it a higher grade of bilious fever. But we, who believe that they are different diseases, and originate from different sources, would have ascribed such an occurrence to the infected vessels, and not to the local causes which had previously existed, and had produced bilious fever. On the other hand, if the yellow fever had been imported to Staten-Island, and prevailed there before the gale, and it had so happened that very shortly after it, the inhabitants who resided near to the Quarantine ground were attacked with bilious fever, although arising from an evident local cause, nevertheless, the medical gentlemen from whom we differ on this subject, might consider such an occurrence as an evidence that the disease arose from the infected vessels driven on shore in the gale, and as both yellow and bilious fever would prevail there at the same time, they would be considered as one and the same disease. But the facts now before us furnish no pretence to either party for cavilling. As we find no febrile disease prevailing at Staten-Island before the gale, we also perceive that the yellow fever was the only one which took place after it. As bilious fevers are changed into common remittents and intermittents, ought we not to meet with yellow fever changing into those diseases also, if it is only a higher grade of bilious fever? We further observe that bilious remittents, after becoming intermittents, are again converted into bilious remittents, and prove fatal. Does the same principle of transmutability obtain in yellow fever? If it does, I have never been favoured with a view of such transmutation. If the dormant poison, lurking in the system of a person who has bilious fever, can be converted into yellow fever, and if such change depends upon the additional causes of bad air, neglect of cleanliness, want of attendance, and of suitable remedies, then I know of no place more likely to accomplish this than the confined forecastles of coasting vessels. Many persons with bilious fever are annually received into the Lazaretto, under the above circumstances; yet I have never met with yellow fever cases, in such vessels, during sixteen years residence at the Marine Hospital, on Staten-Island, except when it had previously prevailed at some southern port.* Although bilious fever has been nearly as fatal, some seasons, as yellow fever usually is, yet I have never seen it terminate in the black vomit, properly so called.

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* One or two cases of remittent, and not one of bilious, fever occurred within a mile of the Quarantine ground, the whole season.

Hard or black frost has been long observed to destroy the contagion of yellow fever, but not to put an end to bilious fever. Can this difference be accounted for, on the principle that the former is only a higher grade of the latter? Is it usual for an agent that removes the greater effect of any cause, not to put as effectual a stop to a lesser degree of the same cause? Such an occurrence is contrary to the common course of events, and requires a reasonable explanation, before we can consider those fevers to be one and the same disease. The contagion of yellow fever has been dissipated even in warm weather, when the foreign source has been extinguished either by purification or removal; and the inhabitants of such district have returned to their houses with impunity during warm weather, after the purification was accomplished. Since experience has taught us that this fatal malady can be arrested in populous places by deserting the infected spot, its ravages have become limited. If the same measures had been pursued in this city in the years 1795 and 1798, and 1803 and 1805, as in the year 1819, it is probable that an equally favourable result would have ensued. On the principle of contagion, this great difference between the number who had this disease in the former and latter periods, may receive a satisfactory explanation. The inhabitants remaining in an infected district, would daily become sick, and contaminate the atmosphere with its specific contagion. This would produce the fever in others, and hence its sphere of action would be constantly extending its limits far beyond its original bounds. If the yellow fever was of local origin, and non-contagious, could its limits be extended by the inhabitants remaining and falling victims to it; and would it be arrested in its bounds by the removal of the same inhabitants? A mere local cause, producing a non-contagious disease, could not affect the inhabitants residing beyond the limits of its operation; and whether the inhabitants of such infected district remained or removed from it, they could not have the least agency in extending it. But in the instances above referred to, the yellow fever, beginning at a point, daily extended its baneful influence, when the inhabitants only partially removed, until upwards of two thousand fell victims to it, in the year 1798: while in the year 1819, by the timely removal of the inhabitants, only about sixty persons died, and it did not extend beyond the originally defined limits.

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* It is very seldom that cases of yellow fever are brought from our southern ports; for as soon as the disease spreads there to any extent, business is so much depressed in those towns, that the intercourse between them and other places becomes very limited. Not one case was received from Savannah at the Lazaretto, in the year 1820, when it proved so fatal at that city. But from the 17th of August, about which time the malignant fever began at Savannah, to the 16th of October, only ten vessels arrived here from that port, and they were less sickly than vessels coming from that place usually are at that season, in years when Savannah is considered to be healthy. This singular exemption from disease was attributed to the seamen remaining on board of their vessels, which lay at a distance from that part of the city where the fever prevailed, and to the shortness of the time that those vessels remained in that port.

Fourthly. *Yellow fever, like most of the specifically contagious diseases, makes its appearance in a fixed period, after the contagion is received into the system.* By reviewing the twenty-nine cases herein enumerated, we are furnished with too many facts where this disease has been taken, after a given period, to suppose that it could be the result of mere accident; but it must be considered as one of the attributes of this disease, and a strong presumptive evidence of its specific character. Ten cases of the twenty-nine sickened on the fifth and sixth days after their first exposure to the contagion, and eleven more were taken ill at the same period of time, after they were placed in situations more immediately under the influence of its contagion than they previously had been, as particularly described in each case. To this number may be added the only two persons who contracted this fever the last summer, by shipping on board of infected vessels at Quarantine, one on the fifth, the other on the sixth day after they embarked. The youngest son of Mr. Daniel Van Duzer was also seized with this disease, the sixth day after his brother was taken sick, and is another instance of personal contagion taking place in the pure air of the country.* These twenty-six cases make more than five-sixths of the whole number who were taken ill with yellow fever, at Staten-Island, the last season.

It appears from the occurrences of the last season, as well as of former seasons, that the contagion of yellow fever may lie dormant in vessels for a long time, and not be brought into active operation, until elicited by a continued warm and congenial temperature, or some violent concussion of the vessel.

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* Mr. Daniel Van Duzer resides near half a mile from the Lazaretto. His eldest son, who was master of a lighter, was taken sick with yellow fever, the 14th, and died the 18th of August. He, no doubt, contracted his disease while lying alongside of infected vessels, and receiving their cargoes. His youngest brother, who was frequently about his bed, during his illness, was taken with the same disease, on the 19th of August, which was about three weeks before the disease prevailed at the Quarantine ground; and as he had not been near to any infected vessels, his disease cannot be accounted for in any other manner than by personal contagion.

I have thus endeavored to furnish a faithful narration of facts, as they have occurred at the Quarantine ground, and have offered some observations which appeared to me to have an immediate bearing on this important subject. The practical inference to be drawn from them, is, that rigid Quarantine regulations are essentially necessary to guard the inhabitants of our commercial cities against the introduction of pestilential diseases.

New-York, February, 1822.

ART. III. STRICTURES *on the late Circular addressed to the Citizens of the United States by* JAMES SMITH, M. D. Agent of the Vaccine Institution, at Baltimore: communicated in a Letter to the Editors, by HENRY W. DUCACHET, M. D. of New-York.

March 9th, 1822.

GENTLEMEN,

THE experience of twenty-four years* has verified the efficacy of vaccination as a prophylactic against the small-pox. Notwithstanding the obstacles which every discovery must encounter in the ignorance, the prejudices, and the interests of

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* I say twenty-four years, calculating from the time that Dr. Jenner promulgated his discovery. Without wishing to detract from the merit of this excellent man, I think there is some reason to believe that the kine-pock inoculation was known and practised before his time. The following extract from the last report of the Royal Jennerian Society, would seem to show that it was familiar to the ancient Hindoos.

"We derive the following from the *Madras Courier* of the 12th January. It is the conclusion of a letter, of which the former part contains a learned exposition of the *spasmodic cholera*, collected from works in general use among Hindoo medical practitioners, with the formulas of the medicines prescribed. The subject incidentally introduced merits all the prominence which it will receive by being detached.

"'As my examination of the Vaidya Sástras has been casual, and may never be repeated, I shall here notice a fact, which will add another to the many proofs of the truth of the wise man's adage, that 'there is nothing new under the sun:' it is, that the inoculation for the cow-pox was known of old time to the Hindu medical writers. To substantiate this men; notwithstanding the determined opposition which it was, in a remarkable degree, the fate of vaccination to endure; the fact, that the vaccine disease is a preservative from the small-pox, commands the faith of the whole world, and is received as

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statement, it is necessary only to refer to the *Sactéya Grantham*, attributed to Dhanwantari, and therefore undoubtedly an ancient composition. In this work, after describing nine several species of the small-pox, of which three (one, Alábhi, being the confluent kind) are declared incurable, the author proceeds to lay down the rules for the practice of inoculation. From this part the following extracts are taken:

" ' TRANSLATION.

" ' Take the fluid of the pock on the udder of a cow, or on the arm between the shoulder and elbow of a human subject, on the point of a lancet, and lance with it the arms between the shoulders and elbow until the blood appears; then mixing the fluid with the blood, the fever of the small-pox will be produced.

" ' The small-pox produced by the fluid from the udder of a cow (Góstany' odacum) will be of the same gentle nature as the original disease, not attended by fear, not requiring medicine; the diet may be according to the pleasure of the patient, who may be inoculated once only, or two, three, four, five, or six times. The pock, when perfect, should be of a good colour, filled with a clear liquid, and surrounded by a circle of red; there will then be no fear of the small-pox as long as life endures. When inoculated with the fluid from the udder of a cow, some will have a slight fever for one day, two, or three days, and with the fever there will sometimes be a slight cold fit; the fever will also be attended by a round swelling in the armpits, and the other symptoms of the small-pox, but all of a very mild nature. There will be no danger, and the whole will disappear in three days.

" ' CALVI VIRUMBON.' "

" ' *Madras, Jan.* 2, 1819.' "

However faithful this extract from the Shastra, and however ancient this book may be, it is well known that the Bramins resisted the introduction of vaccination into India, with the most malignant pertinacity; and that they propagated the most wicked rumours against the British surgeons who recommended and practised the operation. It was only by *bribing* the Indian inoculators, that they could be persuaded to use the vaccine virus. If vaccination was ever known among them, the knowledge of it had probably been lost.

The following extract I find in my common-place book; but having inadvertently omitted to note the book from which I took it, I cannot state one of those unquestioned truths, upon which a doubt cannot reasonably be entertained. When we consider the importance of confirming and maintaining this happy confidence in vaccination, and calculate the inestimable blessings it has conferred upon the world,* it is painful to witness an occurrence which may tend, in the least degree, to impair this confidence. How much is it to be regretted, then, that a public officer, commissioned to preserve and perpetuate this invaluable prophylactic, and in whom we had a right to expect a constant and zealous defender, should, for the inexcusable purpose of justifying an unfortunate mistake, have addressed an imprudent circular to the public, calculated directly to subvert their faith in the virtues of vaccination! It seems that in November last, Dr. James Smith, the Superintendant of the General Vaccine Institution, at Baltimore, transmitted to Dr. Ward, of Tarborough, North Carolina, a

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the authority upon which it rests. "In the year 1769, kine-pox was described (in a weekly paper, Allgemaine Unterhalturgen, published at Gottingen,) as a well known disease 'here in this country (Germany) which infects persons who attend the dairies and prevents the infection of small-pox.' Dr. Jenner published his 'Inquiry into the causes and effects of Variola Vaccina,' &c. in 1798, twenty-nine years later."

* Every body has heard and read of the dreadful ravages of the small-pox, at various periods, since its appearance in the world. But few have a true idea of the advantages of vaccination; and many are inclined to ascribe the less freqnent appearance of small-pox, of late years, in a great degree, to the practice of *variolous inoculation.* The fact is, however, that the old inoculation for small-pox, although it mitigated the violence of the disease, in individual cases, actually increased the mortality from this plague. Dr. Heberden has shown, by accurate calculation, that after the introduction of the inoculating process into London, ninety-five deaths out of a thousand, were caused by small-pox; whereas before, the average was only *seventy.* What a miserable expedient inoculation was for the extermination of the small-pox, may be determined from the fact that this disease continued *annually* to destroy no less than *fifty thousand* of the inhabitants of Great Britain! For further evidence on this point, see Walker's Inquiry into the Small-pox, p. 452. Baron Dimsdale's Thoughts on general and partial Inoculation, p. 23. note. Lettsom's Medical Memoirs, &c. p. 164. Moore's History, &c. of Vaccination, p. 134. But vaccination is never fatal.

parcel containing the matter of small-pox, instead of the vaccine infection. The consequence was, that this dreadful scourge was extensively introduced among the unsuspecting inhabitants of that village, and committed dismaying ravages in the surrounding country, before the proper remedy could be procured to arrest it. To escape the odium which such an occurrence necessarily brought upon him, Dr. Smith, with an imprudence wholly inexcusable and unaccountable, except upon the charitable supposition of his being overwhelmed by the consternation of the moment, addresses a circular to the people of the United States, in which he attempts to account for the accident in a manner which goes directly to subvert all confidence in the prophylactic virtue of the vaccine virus.

I beg leave to offer to you a few strictures upon the doctrines and principles maintained in this singular communication, and to solicit for them a place in your contemplated journal.

To account for the introduction of the small-pox into Tarborough, he has recourse to the following positions:

"1st. I have been unavoidably engaged in attending to persons diseased of the small-pox here. I daily visit, handle, and administer medicine to them; and although I have always taken every precaution that I knew was necessary to prevent this contagion adhering to my person, still it is possible that such an accident may have occurred.

"2d. The genuine kine-pock is in itself so nearly allied to the small-pox, that when this plague becomes epidemic in any place, it intermixes with the vaccine matter by a natural and unavoidable process, and in a manner that may possibly elude the utmost care and vigilance of any person to prevent it.

"3d. The same persons or subjects from whom the vaccine matter must be taken, (for it cannot be otherwise procured than from persons who are fit subjects for it,) may be the bearers of the genuine kine-pock and of the true small-pox at the same time.

"4th. The same identical kine-pock vesicle, under such circumstances as we are now unhappily involved in, that will one day furnish the most pure and genuine vaccine matter, is susceptible of a change in its natural properties, and within a few days afterwards will furnish a different matter, capable of generating the most destructive small-pox."

"1*st. I have been unavoidably engaged in attending to persons diseased of the small-pox here. I daily visit, handle, and administer medicine to them; and although I have always taken every precaution that I knew was necessary to prevent this contagion adhering to my person, still it is possible that such an accident may have occurred.*"

It is really surprising that the Vaccine Agent should have thought to palliate his conduct by a confession, which, on *his* principles, must forever condemn him. I really cannot perceive how he finds an excuse for transmitting the small-pox to North Carolina, in the acknowledgment that he had placed himself in circumstances which rendered such an accident *possible.* If he really believed it possible for the contagion adhering to his person during his attendance upon patients diseased of small-pox, to be communicated to others by letter, and even to infect the vaccine virus transmitted by post, how can he reconcile it to his professional obligations, to his duty as a public agent for supplying the country with the vaccine virus, or to his conscience as a man, to expose himself by visiting, handling, and administering to any such? To say that he was "*unavoidably engaged*" in such attendance, is no apology. No circumstances can require or justify an exposure, which paramount duty should have taught him to avoid. But I am really at a loss to conceive how the variolous contagion, adhering to the Doctor's person, could have affected the vaccine virus, transmitted to Tarborough. I have never before seen it intimated that the matter of small-pox could infect an inanimate, inorganic substance, so as to convert it into its own nature; and have always considered it as a received fact, that the animal poisons are the exclusive product of animal secretion.

"2*d. The genuine kine-pock is in itself so nearly allied to the small-pox, that when this plague becomes epidemic in any place,* *it intermixes with the vaccine matter by a natural and unavoidable process, and in a manner that may possibly elude the utmost care and vigilance of any person to prevent it.*"

I am at a loss to conjecture what is meant by this alleged *alliance* between the vaccine disease and the small-pox. I presume, however, that the fair interpretation is, that they are *varieties* of the same poison. If this is the Doctor's meaning, it is no difficult matter to refute the assertion; for, independently of the clearly marked differences in their external appearances, and the entirely dissimilar train of phenomena which they exhibit in their course, that they are distinct affections is evident from the small-pox being peculiar to the human subject, and wholly incommunicable to the bodies of any other species of animals; whereas it is far otherwise with the vaccine infection. And perhaps it would not be a very easy matter for the Doctor to adduce a single case in which inoculation with the vaccine lymph has ever produced small-pox; (except in Tarborough!) or the inoculation for small-pox has ever eventuated in the eruption of a vaccine vesicle. It is a well-known fact, too, that the true vaccine disease may be produced in a person who has been the subject of small-pox. Now, where is there an analogy to justify the supposition, that a disease which, after being once contracted, destroys the susceptibility of the body to a second attack, may yet appear in another variety at a subsequent period?

As it regards the assertion, that when small-pox "becomes epidemic in any place, it *intermixes* with the vaccine matter by a natural and unavoidable process,"—it is directly at variance with universal observation. If the fact were thus, why are institutions established for the extension of vaccination? why is so much anxiety felt by the guardians of the public safety, that every individual in the community should undergo this process? why are disabilities imposed in some countries,* upon persons who have not been vaccinated? and why are laws enacted to enforce submission?* It is evident that if this were the fact, the small-pox could never be exterminated, or even arrested. An epidemic small-pox would commit the same destructive ravages now as formerly, before the discovery of the preservative virtue of the vaccine disease. In vain would Dr. Smith invite the citizens of North Carolina to apply to him for the vaccine infection to arrest the progress of the desolating plague!—All his philanthropic efforts would be in vain—nay, worse than useless, since every vaccinated person must be in danger of having this affection transformed into the loathsome disease against which it was intended to secure him, and must thus become a new focus of variolous contagion. It is not necessary to enter farther into a refutation of this singular proposition. Indeed, it is so obviously untrue, as not to deserve serious notice, unless to undeceive those who have been misled by it. Yet as Dr. Smith is very fond of referring to the ravages which the small-pox made in this city in the years 1815 and 1816,† it may not be impro-

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* In France the principals of schools are forbidden, under heavy penalties, to receive scholars who are unprotected from the small-pox. In Holland the schools are shut to all who cannot produce a certificate that they have either had the small-pox, or have been vaccinated. No assistance

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is given to the poor whose children have not undergone this process. And no person is allowed to be married, unless he can produce a physician's certificate of his being unsusceptible of the contagion of small-pox.

* In most European countries it is highly penal to inoculate for small-pox; and in England it is an indictable offence, even to cause a person labouring under small-pox, to pass through the streets. See the case of THE KING versus TAUNTON, in the London Medical Journal.

† In a pamphlet entitled, "Two letters relative to the Vaccine Institution, addressed to the members of the Hon. the General Assembly of Maryland," &c. Dr. Smith makes the following unfounded and ungenerous attack upon the citizens and faculty of New-York. "What a heavenly opportunity existed, at the commencement of this plague, to have put the monster to death in the city of New-York, and to have saved our country from the incalculable injuries which we have sustained! The kine-pock, in the hands of any one attentive citizen who would at that time have made a proper use of it, might have saved the lives of all those who have since been swept off in our view by the small-pox, and who are now lost forever to their country and to their friends. But there was *no individual or society* possessed of a just sense of the approaching danger, to give the alarm. Ostentatious philanthropy was asleep, and there was no one to throw 'a

per to adduce a few facts from that very epidemic, to nullify the assertion that, in a pestilential season, small-pox unavoidably intermixes with the vaccine disease, and converts it into its own nature. During that epidemic, I myself vaccinated in one day ninety persons, at the City Dispensary; and for seven weeks a physician was constantly employed in performing the operation upon all who would come to the institution for that purpose. Of the multitudes who flocked to this fountain of safety, numbers had been driven to it by the seizure of some of their families with the small-pox; and returned immediately to the care of the sick, without receiving any injury from their infected apartments. In not a single instance was the vaccine vesicle known to be transformed into a variolous affection; or even to be changed or modified by constant exposure to the variolous contagion.

It is indeed, gentlemen, apart from the subject and the purpose of my letter, yet I cannot but remark, that Dr. Smith, in permitting himself to say "that there was not in New-York *any*

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single bucket of water to extinguish the flame.' *No seasonable effort was made by any one;* but this plague was *allowed* to take its natural course, and it spread abroad, daily increasing its fury." p. 17. And in a "Prospectus of a permanent Vaccine Institution, to be established in the city of Washington," &c. he uses the following illiberal language in reference to the same occasion. "So little notice was taken of it, and so perfectly *careless of its consequences* were all those who ought to have felt some concern about it, that it was suffered to be propagated without any active opposition, until it became that terrible and destructive plague which has since destroyed the lives of so many of our citizens. The kine-pock, in the hands of any one attentive citizen, who would have made a proper use of it at the favourable moment which here presented itself, might have easily strangled this plague at its birth, &c. But it seems, that there was not then in New-York, any one individual or society, who possessed a just sense of the existing danger. There was no one disposed in mercy 'to throw a single bucket of water to extinguish the rising flame;' &c. How immense this waste of human existence! and how awful the punishment, which has been inflicted upon the people of the United States, if we consider the unlimited extent of the calamity which has since followed for want of that attention which was due to it when first discovered!" p. 13.

individual or society possessed of a just sense of the existing danger;" that "there was no one disposed in mercy 'to throw a bucket of water upon the rising flame;'" and that "no seasonable effort was made by any one;" has been guilty (to use the mildest language which charity can dictate) of a rash and unfounded declaration. It is an undeniable fact, that in New-York, vaccination was practised to a large extent at its first introduction into this country, when the prejudices of the profession, and the clamorous opposition of the populace, excluded it from most of our cities. It was here that the first public institution* was opened for the gratuitous vaccination of the poor, and the perpetuation of this inestimable blessing. And it is here that public munificence and private charity have, in an unparalleled degree, been directed to this noble and important object. On the very occasion when Dr. Smith exclaims against our apathy and indifference to this interesting subject, liberal appropriations of money were made by the municipal authorities, to assist in diffusing the blessings of vaccination; the physicians of the City Dispensary were directed to go from house to house, to vaccinate every individual who required it; a person was employed at the office of the Board of Health for the sole purpose of performing this operation; and repeated and urgent invitations were given, even from the pulpit, to all who were liable to the infection of small-pox, to come and receive gratuitous vaccination at this place.

3d. The third apology Dr. Smith makes for the accident at Tarborough is, that "*the same persons or subjects from whom the vaccine matter must be taken, (for it cannot be otherwise procured than from persons who are fit subjects for it,) may be the bearers of the genuine kine-pock and of the true small-pox at the same time.*" The truth of this proposition must, to a certain extent, be admitted; but it can, in no way, serve the purpose for which it was advanced by Dr. Smith. It has, indeed, been proved, that if a person is inoculated at the same time with the virus of small-pox, and with the vaccine lymph, both diseases will be produced. So far Dr. Smith is correct in stating that a subject may bear kine-pock and the small-pox in his person at the same time. But it has also been proved, that as soon as the *constitutional* affection of one of these diseases is produced, the other disease is suspended;* and that, should the vaccine infection predominate, the virus of the small-pox is extinguished in the system. The same rule holds good in cases where kine-pock inoculation has been performed during the period between the casual exposure to small-pox, and the eruptive fever of the disease. It is only, then, in the first or *local stage* of these affections, that they can exist together in the same constitution. Now, it cannot be supposed that Dr. Smith has ever supplied himself with matter at *this* period of the affection; and, therefore, he could not have unavoidably and unknowingly procured the infection of variola in this way. But even granting that the two diseases may both go on to perfection at the same time in the same system, Dr. Smith will hardly allow us to suppose that, in procuring his matter for vaccination, he, who has been twenty years a vaccinator, was *unable to distinguish* between the vesicles of the vaccine disease and the pustules of small-pox; and that he *ignorantly* supplied himself from the latter. And still less, I presume, would he be willing to grant, that he had been so imprudent as to procure any vaccine matter from a person who at the same moment was affected with small-pox! But the fact is, neither Dr. Smith, nor any body else, has ever seen a case in which the two diseases were in full perfection; and, therefore, this third proposition will not explain the introduction of small-pox into Tarborough.

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* This Institution was established in January, 1802.

" *4th. The same identical kine-pock vesicle, under such circumstances as we are now unhappily involved in, that will one day furnish the most pure and genuine vaccine matter, is susceptible of a change in its natural properties, and within a few days af-*

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* Consult on this subject the " Practical observations on the inoculation of cow-pox, &c. By James Bryce, F. R. S. Ed."

terwards will furnish a different matter, capable of generating the most destructive small-pox."

His fourth proposition is as singular as it is erroneous. In the whole course of my practice in vaccination, (and I think I may justly lay claim to some experience in this department,) I have never witnessed such an occurrence. In all my reading on this subject, and in all my intercourse with the profession, I have never before learnt that the vaccine vesicle was liable to any such deterioration as the Doctor speaks of. Were it possible for the kine-pock vesicle to be transformed into a variolous pustule, or for the dried lymph to undergo such a change of its properties as in a few days to degenerate into a matter capable of producing the most destructive small-pox; how comes it that we have never before been apprized of this by the Vaccine Agent? and how is it that accidents have never, before the unfortunate affair at Tarborough, been known to occur from this source? Well may the Doctor say, that " few practitioners have any idea that in the selection of the vaccine matter, they may be exposed to so much danger from causes of this description." None, I believe, have ever entertained any such idea but himself; nor is it to be apprehended that the Doctor's information will ever give them much alarm.

Had I been aware, at the time I commenced this letter, that Dr. Smith had acknowledged, in a communication to the honourable the Speaker of the House of Representatives of the United States, that he himself had, through mistake, transmitted the matter of *small-pox* to Dr. Ward, in the form of scabs, which he had taken from a variolated patient,* I would probably not have troubled you with these observations. These strictures were written before I received the information. They may serve, however, to undeceive some reader who may never meet with Dr. Smith's acknowledgment. I cannot but take occasion to remark, that it is sincerely to be hoped, that as Dr.

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* See the " Report of the select committee appointed to inquire whether it be necessary to modify or alter the law to encourage vaccination," as received by the House of Representatives, on Friday, February 22d, 1822.

Smith has been appointed the public agent *to exterminate the small-pox*, he will never again preserve its contagion in any form, and thus run the risk of renewing and propagating this dangerous infection on another occasion.

With assurances of high personal regard,

I am, gentlemen, your's respectfully.

HENRY W. DUCACHET.

ART. IV. OBSERVATIONS *on* CRURITIS, *or* PHLEGMASIA DOLENS: communicated in a letter to J. W. FRANCIS, M. D. Professor of Obstetrics and the Diseases of Women and Children in the University of New-York, by DAVID HOSACK, M. D. F. R. S. Professor of the Institutes and Practice of Medicine in the University of New-York.*

New-York, March 11th, 1822.

DEAR SIR,

IN reply to your queries relative to Cruritis, I observe, that since the year 1791, when I commenced the practice of Medicine, several cases of this disease have fallen under my care. The following are the memoranda I made of them; if you consider them as deserving a place in your journal they are at your disposal.

In the *first* case, it occurred under the ordinary circumstances in which the disease appears, viz. a few days after delivery, and before the disappearance of the lochia. It was treated by cathartics, dilution, liniments, fomentations, but without any view to its general inflammatory character: it was of six weeks duration, but ended without any permanent injury to the constitution, or to the affected limb.

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* In the first paper of this Journal notice was taken of a case of Phlegmasia Dolens, under the care of Dr. Hosack. Since that article was written, Dr. Hosack has obligingly favoured the editors with the particulars of that case more at large. In the present communication will also be found some novel views of this disease, and reasons for a different classification than has been heretofore adopted.—*Ed.*

The *second* case that fell under my notice also appeared soon after parturition, and during the flow of the lochia. Viewing the effusion in the limb as bearing some resemblance to dropsy, I had recourse, after prescribing an active cathartic, to a combination of calomel and squills, one grain of each administered three times a day. This prescription was attended with a manifestly favourable result, removing the disease in a few days.

In a *third* case, occurring under similar circumstances, the disease, after being confined to one limb for several days, appeared in the other, but was removed from both by the cathartic and diuretic operation of the calomel and squills.

The *fourth* case that offered, gave me a much more enlarged view of the nature of this complaint. The lady who was the subject of it had been confined twenty-three days at the time of her attack; the lochia had also entirely disappeared ten or twelve days; she had undergone a severe labour, having borne twins, and during her pregnancy experienced much inconvenience from her inordinate size.

She was first affected by cold producing catarrhal and pneumonic inflammation; but within forty-eight hours a metastasis took place to the limb, which proceeded to swell with all the symptoms of idiopathic cruritis, the affection of the lungs totally disappearing. By general blood-letting, saline cathartics, antimonials, tepid applications to the part affected, with a strict antiphlogistic diet and regimen, the first stage of the disease was in a few days removed; afterwards, by stimulating liniments, frictions, and the roller, the parts affected were restored.

A *fifth* case presented itself in the course of the last winter: In this lady it was induced by the excitement arising from the imprudent use of wine recommended by the practitioner who delivered her; her labour had been of short duration, and in all respects natural. By the antiphlogistic treatment this case also ended favourably in a few days.

Sixth. A lady who had been many years since affected by rheumatism in the left limb, after her confinement in which I attended her, became the subject of *cruritis*, affecting the same limb that before had been the seat of suffering; her labour was a natural one. In this case her disease was the effect of cold, induced by an imprudent change of dress. When the limb was first affected, she considered it as a renewal of her former attack of rheumatism, which before had been altogether unconnected with parturition. In a few days it yielded to the same treatment I have above inculcated, and which I have more fully detailed in my observations on this subject, contained in the appendix to the last edition of Thomas's Practice of Physic, and in my notes subjoined to the article *Medicine* in the American edition of Brewster's Edinburgh Encyclopædia.

Seventh. I prescribe for a lady, the mother of a large family of children, who has suffered four attacks of this disease after parturition. The complaint in every instance took place within nine days after delivery, commencing with pain in the calf of the leg. It always affected the same limb, and has left the ankle permanently thickened, enlarged, and painful under ordinary pressure.

Eighth. In December, 1820, my son, Alexander E. Hosack, who is engaged in the study of medicine, delivered a poor woman, in whom the disease appeared as early as the second day after delivery. In this case the inflammation and swelling took place in both limbs simultaneously. The labour had been natural, and of short duration. The woman was of plethoric habit, insomuch that delirium took place in the latter stage of her labour. The patient was situated in a cold uncomfortable apartment, which was considered as the exciting cause of her disease. This case also yielded to the lancet, and other means of depletion.

Ninth. During my last tour of duty in the winter of 1821 and 1822, at the New-York Hospital, a very similar painful enlargement of the limb occurred in a man who had been admitted as a fever patient. In a few days the left leg and thigh became the seat of inflammation, attended with a swelling of the whole limb, exhibiting the remarkably glossy whiteness and œdematous appearance that haracterisec cruritis in the female, and attended with similar pain and sensibility to the slightest movement. The patient had been so much reduced and emaciated by the preceding fever and the means prescribed, that upon the appearance of this local affection, the usual evacuations by the lancet were in his case altogether inadmissible. The only remedies employed were such as were calculated to allay the sufferings of the limb, and to moderate the general febrile action of the system. It ended in an extensive suppuration in the thigh, followed by hectic fever, and ultimately proved fatal.

From the foregoing cases the following inferences may be deduced:—

1st. That cruritis is an inflammatory disease, not only affecting the limb, but the whole system.

2d. That it most usually proceeds from a suppression of the natural excretions, the effect of cold, stimulating drinks, and other means of excitement.

3d That it is not necessarily connected with the lochial discharge, as inculcated by Trye, Denman, and indeed by Rodrigus Decastro, of Hamburgh, in 1603, by Wiseman, in 1676, and by Mauriceau, in 1712, who were the authors of this doctrine.

4th. That the first irritations frequently appear about the calf of the leg, and not in the groins and pelvis, as asserted by Dr. Denman.

5th. That it follows easy as well as difficult labours, and therefore cannot proceed from the pressure of the child's head upon the edge of the pelvis rupturing the lymphatics, as supposed by Mr. White.

6th. That it is not a disease confined to the lymphatics, but as in the cases recorded by Dr. Hull, it appears in every part of the affected limb.

7th. That it is not confined to females, but, as in the cases recorded by Dr. Hull, Dr. Ferriar, Dr. Thomas, and others, it occasionally appears in males.*

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* See Medical and Chirurgical Journal for 1817. Medical and Chirurgical Transactions for 1819.

8th. That as in gout and rheumatism, when depletion is not actively employed, the inflammation, after appearing in one limb, is in some cases transferred to another.

9th. That it sometimes appears in both limbs at the same time.

10th. That the general means of subduing inflammatory action are the most effectual in removing the *active* stage of this complaint.

11th. That in the second stage of cruritis, in addition to the use of general stimuli and tonics, stimulating spirituous liniments, friction, and the roller, are most useful in restoring the circulation, and in exciting the absorbents in the removal of the swelling which remains in the passive stage of this disease.

12th. That occasionally, as in the cases related by Hull, Denman, and by Zinn,* it ends in abscess, and proves fatal, especially where the antiphlogistic treatment has not been vigorously pursued in the first stage of the disease, or when it occurs under great exhaustion and debility of constitution.

In compliance with your request, I will with pleasure make you occasional communications of a practical nature.

With my best wishes for the success of the Journal you are about to commence, I am, Dear Sir, with great regard, yours,

DAVID HOSACK.

ART. V. A CASE *of* TUMOUR *within the* PELVIS, *preventing parturition.* By JACOB DYCKMAN, M. D.

THE records of medicine afford sufficient evidence of the occasional occurrence of tumours within the pelvis, obstructing or entirely preventing parturition. Fatal consequences may ensue from such incidents, and it is therefore of importance that the attention of practitioners of midwifery should be directed towards this interesting subject. The following case, which has fallen under my observation, and which appears to differ in some respects from any that I have yet seen recorded, will not, it is hoped, be thought uninteresting.

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* See Gottingen Transactions.

In August, 1820, I was called to a woman, at the full period of her third uterine gestation, who had been in labour a considerable time. I found her under the care of a midwife, who reported that the waters had been discharged, and that she could plainly feel the head of the child presenting. On examination, *per vaginam*, I soon discovered that what the midwife had mistaken for the head of the child, was a large and firm tumour pressing laterally and posteriorly against the vagina, and which appeared to be situated partly between it and the rectum, but chiefly inclined to the right side of the pelvis. It intrenched so much upon the capacity of the pelvic cavity as to render impracticable all attempts to ascertain the state of the os uteri. Efforts were made to dislodge the tumour from its situation by pressing it in different directions, but in vain; it appeared to be firmly attached to the vagina and lower parts of the pelvis. Its nature could not be ascertained; but I was induced, from several circumstances, to believe that it could not be ovarian; nor could it be owing to a rupture of the cervix uteri and protrusion of some part of the child into the pelvis, for none of the symptoms which give evidence of such an occurrence were present; *and the labour pains continued regular.* The tumour did not yield or diminish by the force of the pains, and every attempt to displace it had proved ineffectual. I was therefore convinced of the impossibility of the child being born, without artificial assistance. But as the patient was much exhausted, and did not appear to be in immediate danger, I conceived it unnecessary to take any immediate steps towards advancing the delivery. A large anodyne was administered for the purpose of allaying the pains, and quieting the irritations of the system.

The anodyne had, in part, the desired effect, and the patient got some sleep, by which she was much refreshed. The pains returned with considerable force, and protruded the umbilical chord, which, by its pulsation, gave evidence of a living child in the womb. After some little perseverance, I was enabled to pass my finger sufficiently far along the left side of the tumour to ascertain the presentation of the child's head, which was resting upon the upper part of the tumour.

It was evident that any further delay would only be productive of additional hazard, and that the safest mode of proceeding would be to puncture the tumour, and diminish its size, so as to allow the passage of the child. Drs. Francis, Hosack, Kissam, and Moore, who were invited to see the case, concurred with me as to the propriety of such a course. Preparations were accordingly made for the performance of the operation forthwith.

The patient being placed in a proper position, the tumour was punctured through the vagina, about an inch and a half within its external verge, but no discharge followed. The incision was then extended, and by means of proper instruments the tumour was broken down, and a considerable portion of it, consisting of fleshy and bony substance mixed with blood, was removed. The os uteri, which could now be felt without difficulty, was found to be fully dilated, and the tumour appeared to be sufficiently diminished to allow the child to pass. But the natural efforts of the mother being of little avail, on account of her great exhaustion, and the child being dead, as was evident from the pulsation in the chord having ceased, it was judged expedient to employ the perforator, and deliver with the crotchet. A full-sized child was brought away, after which the woman was put to bed, and made as comfortable as circumstances would permit.

The recovery of the patient was tedious and interrupted. The remains of the tumour were discharged by suppuration. A number of pieces of bone came away, and among others some *perfectly formed vertebræ* of the neck and back, and several *metatarsal bones*.

It seems evident from the details that have been given, that the tumour within the pelvis preventing the delivery of this woman, consisted of an extra-uterine fœtus, which had acquired a considerable size, and filled the greater part of the pelvic cavity. No other means of effecting the delivery could have been used than the one resorted to, except the Cesarean operation, which would have been justifiable, if the tumour had been incapable of diminution by puncture.

ART. VI. OBSERVATIONS *on the use of* EMETICS *in* CONSTIPATION *of the Bowels:* communicated in a letter to JOHN B. BECK, M. D. by DAVID HOSACK, M. D. F. R. S. Professor of the Institutes and Practice of Medicine in the University of New-York.

New-York, March 15th, 1822.

DEAR SIR,

IN the treatment of constipation of the bowels, it appears to be generally agreed upon by the profession, that when we fail to evacuate the intestines by the various cathartic remedies ordinarily prescribed for this purpose, the last resource is mercury carried to the point of salivation; at the same time trusting to opiates and antispasmodics to allay the pain, and to the usual antiphlogistic means of subduing any inflammation of the bowels that may be induced in the course of the disease. In the year 1796, I communicated to Dr. George Pearson, of London, a remarkable instance of constipation yielding to this treatment, after twenty-one days' continuance of the disease.* Reflecting upon the manner in which mercury operates in relieving cases of this nature, after the gums have become affected, it occurred to me that the action of this metal is not only by its effects upon the secretions in general, but more especially upon the biliary organs. The dark coloured and acrid discharges that succeed to this operation of mercury, appear favourable to this explanation. In like manner the general derangement of the digestive organs, the foul tongue, the offensive breath, the sallow complexion, that usually precede and attend upon the first stage of this disease, no less point to this obstructed and torpid state of the liver, as having great agency at least in predisposing to the disease in question, while a check of the perspiration and certain articles of diet, are not unfrequently the exciting causes of irritation and of the spasm and inflammation which ordinarily constitute an attack of this complaint. With these views I was led to the use of emetics as best calculated to remove the hepatic obstruction which appears to lay the foundation of this disease, while by their febrifuge and antispasmodic operation they are no less useful in removing the fever, the inflammation and constriction that constitute some of the most distressing as well as dangerous symptoms that attend a constipated state of the belly. The cases which have fallen under my care, since the adoption of this mode of treatment, are so favourable to the views suggested, that agreeably to my promise, I now submit them to your consideration, and if you deem them deserving more public notice, have the goodness to give them a place in your Journal.

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* See Duncan's Annals of Medicine for the year 1796, vol. i. Edinburgh.

1st. Mr. W. B. of Dutchess county, who had suffered a very severe and dangerous attack of constipation, of which he had been relieved by mercury, prescribed by the late Dr. Bard, after a great variety of the ordinary cathartics had been administered in vain, when on a visit to New-York, was seized with the same symptoms of distress and obstruction of the bowels that appeared at the commencement of his first attack. I observed his countenance to be sallow, his tongue covered with a yellow sordes, his breath offensive, his stomach disturbed, but the arterial system not yet sensibly affected. The ordinary cathartic medicines being rejected, I gave him an emetic of tartarized antimony. It operated freely, producing a general relaxation of the system, with some discharges by vomiting of bilious matter, which, from its viscidity and dark colour, appeared to have been long pent up in the gall-bladder: they were succeeded by copious evacuations by stool. He was instantly relieved, and was himself sensible that by this timely prescription he had been saved from a renewal of the same sufferings he had experienced in his first attack.

2d. Mr. S. W. a merchant of this city, whom I had frequently attended in very severe and dangerous attacks of constipation, and which only yielded to mercury, administered to the point of salivation, some years since experienced a renewal of the symptoms that usually had announced a visitation of his disease. I found him in great pain, his bowels in a state of violent constriction, his pulse small, his countenance sallow and livid: I gave him instantly fifteen grains of ipecacuanha combined with two of emetic tartar. It operated freely, both by vomiting and by the bowels. He was immediately relieved, and had no return of the obstruction.

3d. Mr. W——e, a gentleman of the bar, who had also been frequently my patient under obstinate constipation, and had been relieved by blood-letting, blisters, the warm bath, enemata, and mercury, given to the extent of salivation, experienced one of his usual attacks. I gave him an emetic of ipecacuanha and tartarized antimony. It also had the same good effects of restoring the biliary evacuations, and of removing the intestinal obstructions, without having recourse to the ordinary offorts that had been directed on former occasions. I have in like manner relieved the same gentleman, by an emetic in a second attack of a similar nature.

4th. The late lieutenant Walter Stewart, a highly meritorious officer of the United States Navy, had suffered numerous attacks of this disease in Philadelphia. In some of those he had been relieved by blood-letting, the warm bath, and injections of tobacco smoke, as directed for him by the late Dr. Wistar, but in most the obstruction was not removed until recourse was had to mercury, administered to the point of salivation, and which, in his case, required a much longer continuance of the medicine than is usually necessary to produce that effect. When in New-York he twice experienced a return of the same disease; in both instances I directed for him an active emetic, which afforded him immediate relief, by producing copious bilious evacuations from the stomach, followed by large discharges from the bowels.

5th. Miss L—— —— who is constitutionally inclined to a torpid state of the intestines, and is very liable to obstinate attacks of constipation, has been three times under my care in this disease After directing in vain the various cathartics ordinarily prescribed, and all the usual forms of enemata, I at length directed the emetic before mentioned : it was followed by immediate relief, although the disease had been of several days duration. Upon another occasion I directed the emetic in the commencement of her complaint, with the same successful result. The same young lady a short time since experienced a return of the same disease, but attended with more severe pain than she had suffered upon any preceding occasion. Being confined to my bed, I requested an eminent physician to visit her; he found her bowels had been confined about seven days, attended with symptoms of intestinal inflammation. After letting blood from the arm, he directed fomentations to be applied to the abdomen, and enemata variously composed to be administered, and advised castor oil, salts, calomel and jalap, in succession, but all to no purpose. The purgative medicines were rejected by vomiting; the enemata were returned as they were administered, without containing the least portion of fœcal matter. She earnestly solicited an emetic to be given her, urging that she had been relieved by this means in both of her former attacks; this was declined by the physician, who considered it to be a hazardous practice to prescribe emetic medicines in a case of this nature. Recourse was had to calomel and opium; salivation was the result, but without the relief by the bowels which usually follows this operation of mercury. Upon my recovery I visited her; she still suffered severe returns of pain : her mouth had been sore three or four days, but notwithstanding the use of enemata which had been frequently administered as fomentations to the bowels when her pain was most severe, no alvine discharge whatever had taken place. Although fifteen days had elapsed since the commencement of her obstruction, I gave her an active emetic, viz. fifteen grains of ipecacuanha and two of tartarized antimony, directing it to be repeated at the expiration of an hour if necessary: the first operated very freely, discharging a large quantity of dark-coloured bilious matter by vomiting, followed by similar copious evacuations by stool. In twenty-four hours she was so entirely relieved that my attendance became almost unnecessary, except for the removal of the soreness of the mouth that remained from the mercury that had been prescribed; for this purpose a gargle of yest, honey, and borax, was directed, with occasional small doses of calcined magnesia, to preserve the bowels in a soluble state.

6th. Captain A. who had contracted, in the East-Indies, a disease of the liver, has recently been affected by a constipated state of the belly, attended with spontaneous vomiting; finding no relief from injections, and all cathartic means given by the mouth being rejected, I gave him an emetic. As in the former cases, it was followed by a general relaxation of the system, and plentiful discharges both by the stomach and bowels.

7th. In another case of a young gentleman, who had experienced many severe attacks of constipation, attended with high inflammatory symptoms, in which, in addition to the use of mercury it had been necessary to make repeated use of the lancet and other means of counteracting the attending inflammation, I have also recently directed emetics with the best effects not only in obtaining discharges from the stomach and bowels, but also in removing the pain, the spasmodic constriction, and febrile symptoms, which have usually attended every return of his disease.

From these facts I have been led to the following conclusions.

1st. That a constipation of the bowels is usually attended with, and frequently produced by, a torpid state of the liver, and consequent deficiency of the biliary discharge.

2d. That the pain, spasmodic constriction, and inflammation, attendant upon this disease, are the result either of the mechanical obstruction occasioned by the deficiency of bile, and consequently a retarded peristaltic movement of the intestines, or the effect of a sudden change of perspiration, or of a particular article of diet.

3d. That in the commencement of constipation, or in its more advanced stage, when the symptoms of inflammation have been subdued by the lancet, emetics may be very advantageously exhibited both for the purpose of removing the hepatic obstruction and of counteracting the spasmodic constriction and pain ordinarily attendant upon this disease.

4th. That the salutary effects which have been occasionally derived from injections of tobacco smoke are attributable to the general relaxation, the nausea, and in some cases, the vomiting, which that narcotic produces.

5th. That the benefits that have in like manner been obtained in some cases from the use of tartarized antimony, administered by injection, are to be accounted for by the nausea and vomiting that have been the effects of its operation, but which are to be obtained with more certainty from the same medicine given by the stomach, and to the extent of full vomiting.

In like manner, inasmuch as a torpid state of the liver and a diminished secretion of bile are generally known to constitute a part of the proximate cause of dysentery,* we obtain a satisfactory solution of the salutary operation of injections of ipecacuanha in that disease, as advised by Dr. Thomas Clark;† but which effects are more certainly to be obtained from the use of ipecacuanha given by the stomach, as prescribed by Sir John Pringle, or of tartarized antimony, as recommended by Senac.

With the hope that these facts, and the principles deduced from them, may lead to a more satisfactory view of the pathology and treatment of the disease which is the subject of this communication, I am, Dear Sir,

Respectfully and truly your's,

DAVID HOSACK.

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* See Drs. Monro, Cleghorn, Pringle, and Girdlestone.

† Observations on the diseases of the East and West Indies, by Thomas Clark.

ART. VII. REMARKS *on* PHOSPHORIC ACID *in the treatment of* JAUNDICE, in a letter to Dr. JOHN W. FRANCIS, from CALEB MILLER, M. D. of Rhode-Island.

Bristol, (R. I.) March 14th, 1822.

SIR,

IT is now about seven years since I first began to administer the phosphoric acid in Jaundice. On my first having recourse to it, I gave it in small doses, being apprehensive of its deleterious effects upon the human constitution: but experience has convinced me my fears were groundless.

I was induced to make my communication to Professor Silliman on the subject of this acid, from a conviction that it was the most useful medicine I was acquainted with in that disease, and that it was likely to prove so in the hands of others. I should have made known this practice sooner, had it not been that I thought I might have been mistaken as to its operation; but I delayed until I was fully convinced of its sanative powers in that affection.

Since my letter to Professor Silliman, I have had under my care several severe cases of jaundice, occurring in old as well as young persons, and they have all readily yielded to the acid.

In my paper in the American Journal of Science and Arts, I might have entered into more minute detail, but it seemed the less necessary as the phosphoric acid is a medicine not particularly dangerous, and any small variation in the dose could do no harm, as I think the stomach the best guide in the administration of the remedy in the disease. I am in the habit of prescribing the acid in balm tea, made agreeably sour, the patient to drink of it as much as the stomach will bear, until it operates as a diuretic, and this effect will generally take place within the course of six hours, and often sooner. In the course of six hours more the urine will be changed to a light colour, when the acid is to be used more sparingly until the urine assumes its natural state: this effect generally takes place in about three or four days.

In my letter to Professor Silliman I have observed:

"About six years ago I had a very obstinate case that resisted the common remedies. I was led to use the phosphoric acid on the principle that acids decompose the bile. I made choice of this on account of its existing in a separate state in the blood.

"I directed a large spoonful of the acid as prepared in Murray's Materia Medica in a pint of balm tea, to be taken as fast as the stomach would bear it, till it should operate as a diuretic. In twenty-four hours the patient had taken eight pints, and it had operated powerfully as a diuretic. Neither the urine nor the white of the eye was as yellow as before, by a very obvious difference. I ordered a continuance under the same directions, and in two days more the urine was nearly the natural colour; but the skin had not improved in the same proportion. I advised tonics, with the occasional use of the acid, and my patient shortly recovered.

"I have had many of the same complaint since that time, and have directed it nearly in the same manner, according to the age and condition of the patients, and the result has been the restoration of health in a very short time. In general, the yellowness disappeared in three or four days from the urine, but continued a little longer on the skin; by the use of tonics, and sometimes a little of the acid, this is however removed in a few days. I have met with only one patient, whose symptoms have not yielded to the above plan. This was a person eighty years of age. Even in this case, however, the acid always produced relief; but the complaint soon returned. My present practice is to give a cathartic of calomel and jalap, or some of the neutral salts, and then the balm tea moderately acidulated with the phosphoric acid, which I direct to have continued till it operates as a diuretic and until the urine becomes clear, or nearly so; this commonly takes place in the course of two days. I have advised other acids when this has not been at hand; but I am inclined to give the preference to the phosphoric, although I think the others deserve a farther trial."

Let me further add, that I recommend this acid in all cases of debility where I would advise any other acid, and that I entertain a high opinion of its tonic powers.

I remain, yours, &c.

CALEB MILLER.

ART. VIII. DESCRIPTION *and* ANALYSIS *of* GIBBSITE, *a New Mineral:* by JOHN TORREY, M. D. of New-York.

[Read before the Lyceum of Natural History, March 18th, 1822.]

AMONG a promiscuous collection of minerals I lately received from the Northwestern parts of Massachusetts, I observed one, which, from some uncommon characters it possessed, I was induced to submit to a minute examination. It occurs in a neglected mine of brown hæmatitical iron ore, in the town of Richmond in Massachusetts, where it was first observed by Dr. Ebenezer Emmons, of that place.

This substance was by some supposed to be a variety of Arragonite, and by others, phosphate of lime; but a few experiments proving it could not be either of these minerals, an analysis was undertaken, in order to ascertain its nature with certainty. Professor Dewey, of Williams College, was engaged with its analysis at the same time with myself, and we agreed very nearly in our results, so that it is doubtful to whom the credit of priority is due. The Professor first announced it as a variety of *Wavellite,* in Silliman's Journal, vol. 2, p. 249; and in vol. 3, p. 239, he gives additional reasons for supposing it to be that mineral. Since, however, Berzelius has lately examined the European varieties of Wavellite, and found their composition so entirely different from what had been stated by Davy and Klaproth, there remained very little doubt but that the Richmond mineral is, what I always supposed it to be, a species not hitherto described.

The detail of the analysis of Berzelius, which is published in the 12th vol. of the Journal de Chymie et Physique, I have not been able to refer to, as the work is not to be had here, and I am therefore ignorant of the means by which the errors of Davy and Klaproth were avoided.

DESCRIPTION OF THE MINERAL.

External characters. It occurs in small irregular stalactitical masses from an inch to three inches in length, and one inch or more in breadth, consisting of an aggregation of elongated tuberous branches, laid parallel, and united together. Sometimes the masses are larger, tuberous, and incrusting.

The colour is dirty white, greenish white, and grayish.

Its hardness is rather superior to that of calcareous spar. It is tough, but easily reduced to powder.

The fracture is indistinctly fibrous, the fibres radiating from an axis.

Its lustre is a little glimmering.

Surface dull.

It is slightly translucent.

Specific gravity, 2.40.

Chemical characters. It does not effervesce with acids. Before the blowpipe it whitens, but undergoes no farther change, even when the heat is increased to the utmost.

ANALYSIS.

1st. One hundred grains in fine powder were heated white hot in a platina crucible for two hours. The mineral underwent little apparent change, but lost 34.7. grains in weight. The same experiment was repeated with other portions of the mineral, and the loss varied, from 34. to 35.5.

2d. Some of the powdered stone was digested in nitric acid. It was speedily dissolved without effervescence, and formed a transparent solution. This had a very astringent sweetish taste, and did not crystallize when reduced by evaporation. A part of the solution was diluted with water, and various tests were applied to it. Sulphuric acid did not disturb its transparency. Ammonia threw down a bulky opaline precipitate. Prussiate of potash, and tincture of galls, produced no effect, neither did the solution of oxalate of ammonia.

3d. One hundred grains of the powder were boiled in sulphuric acid. The whole was dissolved without residue. The excess of acid was driven off by heat, leaving an astringent salt which was soluble in water. This was again made slightly acid. A small quantity of carbonate of potash and some water were added, and heat applied. A transparent solution was obtained, which had a strong taste of alum. It was evaporated and set aside. Well defined octahedral crystals soon formed. These crystals had the peculiar sweetish astringent taste of alum, and intumesced strongly when laid on hot coals. To ascertain whether they were really sulphate of alumine of potash, I dissolved them in water, and added to the solution pure ammonia: an abundant precipitate was thrown down, which was entirely soluble in a solution of caustic potash.

The alkaline solution was neutralized with nitric acid; carbonate of ammonia was then added till precipitation ceased, and the whole thrown on a filter. The earth when well washed and heated to redness for half an hour, weighed 64.8 grains, and had all the properties of pure alumine. It formed soluble and *deliquescent* salts with the nitric, sulphuric, and muriatic acids. It was precipitated from its solutions by potash, and re-dissolved again when these alcalies were added in excess. When a strong solution of potash or soda was poured into a solution of the sulphate, a large quantity of octahedral crystals of alum were suddenly deposited.

From these experiments there can remain very little doubt but that the mineral under examination consists almost entirely of alumine, and something which escapes by the application of heat. As sulphuric acid does not evolve any elastic fluid, and no perceptible vapour is produced when heat is applied, we may safely pronounce this volatile part, *water*.

4th. There being a possibility that the alumine might be united to phosphoric acid, as nearly all the earthy phosphates are soluble in acids, some of the mineral was boiled in water with three times its weight of subcarbonate of potash, the liquid was filtrated, and the residue well washed and collected. This residue, on being tested with acids, appeared to have suffered no change, for no effervescence took place. The filtered liquor, on being *neutralized* with nitric acid, let fall a trifling precipitate which was ascertained to be nothing but alumine. No further change took place on adding muriate of lime. Hence we infer the absence of phosphoric acid.

5th. A portion of the powdered stone was next tested for fluoric acid, by placing it on a piece of glass, moistening it with sulphuric acid, and applying a gentle heat. No erosion could be perceived.

6th. By the preliminary experiments we have proved the absence of iron and lime, but these are sometimes found, especially when the purest specimens are not employed. They never I believe exist in greater quantity than two or three per cent. Magnesia and Zircon are excluded by the test of the caustic alcalies, which do not dissolve these earths. Barytes and Strontian would have been detected, had they been present, by the sulphuric acid in Exp. 2. Tests were applied for the remaining earths, but none were detected.

The results of the analysis therefore are:

Alumine, -	64.8
Water, - -	34.7
	99.5
Loss, - -	1.5
	100.0

The following is the result of the analysis of Wavellite, by Berzelius:

Alumine, -	35.35
Phosphoric acid,	33.40
Fluoric acid,	2.06
Lime, - -	0.50
Oxyds of Magnesia and Iron, -	1.25
Water, - -	26.80

It is therefore a hydrous phosphate of alumine, instead of a *hydrargillite* as it was considered by Davy. The analysis of Wavellite by three different and celebrated chemists is here added for comparison. They are extracted from Jameson's Mineralogy.

	Barnstable Wavellite.		Cornish Wav.	Hualgayoc.
Alumine,	71.50	70.0	58.76	68.00
Oxyd of iron,	0.50		19	1.
Lime,		1.4	37	
Silex,			6.12	4.50
Water,	28.00	26.2	30.75	26.50
Loss,			3.87	
	100.00	100.00	100.00	100.00
	Klaproth, Beit. b. iv. s. 110.	Davy. Nich. Jour. xi. 157.	Gregor.	Klaproth, Beit. b. v. s. 111.

These results stand on such high authority that we should scarcely be inclined to listen to any who should dispute them, except so distinguished a chemist as Berzelius. There can, however, be no doubt but that the Wavellite contains phosphoric acid, especially since its existence in that mineral has been confirmed by the experiments of Professor Fuchs. It appearing from our analysis, that the mineral of Richmond differs in its composition from Wavellite, and its external character being so dissimilar, there can be no hesitation in giving it a proper name, and assigning it a place in the system. The name of GIBBSITE will, I hope, be adopted for this singular substance, in honor of one* who has done so much for the advancement of American mineralogy.

In a chemical arrangement of minerals, Gibbsite must occupy the place of Wavellite, which must now be transferred to the *earthy salts*. The name *hydrargillite* of Davy would be proper for the new mineral, if there would not be danger of confounding two minerals under one appellation. According to the nomenclature of Proust, who established the class of *hydrates* it would be a *hydrate of alumine;* but the name is objectionable, as it conveys the idea of a *salt*, when the compound is not of a saline nature.

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* Colonel George Gibbs.

Nearly all the Gibbsite yet found has been collected at the mine in Richmond, where Dr. Emmons discovered it. It occurs in considerable quantity among the *rubbish;* but the mine being closed, we are ignorant of its precise situation and connexions. No doubt it is produced by infiltration into cavities, for the mineral is evidently stalactitical. It is a little remarkable, however, that so little iron should enter into its composition, as it occurs among *hæmatite*, and is often found attached to it.

REVIEW.

Art. I. Elements of Therapeutics and Materia Medica. *To which are prefixed two Discourses on the History and Improvement of the Materia Medica, originally delivered as Introductory Lectures.* By N. Chapman, M. D. Professor of the Institutes and Practice of Physic and Clinical Practice in the University of Pennsylvania, President of the Academy of Medicine of Philadelphia, &c. &c. Second edition, enlarged and revised. 2 vols. 8vo. Philadelphia, 1821.

Every person of the least discernment must have observed, that within a very few years, medical science has received an impulse in this country, to which it had hitherto been a stranger. The evidences of this fact are so glaring, as scarcely to require any illustration. It may be recognized in the augmented number and increased respectability of our schools of medicine—in the mutiplication of our medical journals—in the intellectual vigour and originality displayed in our medical productions, and in the general intelligence and activity which pervade the profession from one end of the country to the other. In no department of medical science, do we think, has this impulse been felt more efficaciously, or been attended with happier results, than that of the Materia Medica. It is a truth which cannot be denied, that anterior to the revolution but little progress had been made in investigating the character and properties of our native medicinal plants. We could boast indeed of some distinguished naturalists, and of many intelligent as well as learned physicians, who were abundantly qualified for the successful cultivation of this department of medical knowledge. The colonial government, however, was but little calculated to elicit their powers, or to stimulate their ambition. To this cause, among many others, must be attributed the apathy of men, who, under other and more auspicious circumstances, would doubtless have shed a lustre on the era in which they lived. Even after our revolution had been achieved, the excitement occasioned by that great event, seemed for a time to unfit the mind for the calm pursuits of scientific and philosophical research. As was natural enough, men appeared more concerned about the public weal, and were more intent upon erecting and consolidating a system of rational and independent government, than about cultivating literature or science. Almost all the active talent of the country was accordingly enlisted in the service of the state, or at least embarked in that profession which presented the most direct road to political distinction. Hence it happened that medicine, as well as the other sciences, continued for several years in a languishing and neglected condition. With regard to our materia medica, it was not until the time of the late Professor Barton, that any considerable attention was directed to its cultivation. To him is unquestionably due the high honour, not merely of having led the way in the investigation of our native medicinal vegetables, but also of having originated and diffused, to no inconsiderable extent, a taste for the natural sciences generally. He was the first person who attempted any thing like a systematic view of the materia medica of the United States,* and his labours have done infinite honour to himself and to the country. The splendid publications which have recently appeared,† on that subject, are in no small degree, we conceive, indebted for the patronage which they have received, to the direction thus given to the public mind by that distinguished physician and naturalist.

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* See the "Collections for an Essay towards a Materia Medica of the United States, by Benjamin S. Barton, M. D. Professor of Materia Medica, &c. &c. in the University of Pennsylvania."

† See "Vegetable Materia Medica of the United States, or Medical Botany, by Wm. P. C. Barton, M. D. &c. Philadelphia," and the "American Medical Botany, being a collection of the native medicinal plants of the United States, containing their Botanical history and chemical analysis, and properties and uses in medicine, diet, and the arts, with engravings, by Jacob Bigelow, M. D. &c. Boston."

The work of Dr. Chapman is of a different character from any of those just alluded to, inasmuch as it is not confined to the consideration of our own medicinal productions, but takes in the whole range of the materia medica, and contains copious discussions concerning the action of medicines and the nature and cure of almost every disease to which the human frame is liable. It is the first work of the kind which has been produced in this country; and we are proud to say, that its literary, no less than its professional merits will deservedly secure to its author a high rank among the most enlightened and accomplished of physicians. For tasteful and elegant composition—for ingenious speculation, and for general interest, it stands unrivalled by any work of the kind in the English language. In saying this, we by no means wish to disparage the excellent work of Murray. In its way it is admirable, and as a work of *science* is far superior to that of Dr. Chapman. But Murray was no practical physician, and the evidences of this are too apparent not to be observed. His Materia Medica is accordingly extremely defective in the very respect in which, we think, Dr. Chapman's so much excels.

In thus expressing our sentiments concerning the general merits of this production, we do not mean to be understood as approving of all the opinions and doctrines which it inculcates. On the contrary, not a few of them appear to us to be signally erroneous. In an especial manner do we think this to be the case, with regard to a good portion of our author's reasonings concerning the *modus operandi* of medicines. As this is a subject which has been laboured with a good deal of earnestness, and upon which really Dr. Chapman has displayed no small share of ingenuity, we shall take the liberty of examining it somewhat at large. In explaining his views concerning the operation of medicines, our author tells us, " that they *all* act by exciting a local impression which is extended through the medium of sympathy."—vol. i. p. 60. As a corollary from this theory, he denies that any substances ever enter the circulation. "It must be acknowledged," says he, "that no substance, in its active state, does reach the circulation, since it is shown, that a small portion even of the mildest fluid, as milk or mucilage, oil or pus, cannot be injected into the blood-vessels, without occasioning the most fatal consequences."—vol. i. p. 64. To us this doctrine seems singularly inconsistent with known and acknowledged facts. Let us, however, examine some of Dr. Chapman's arguments in support of it. A prominent argument, and one which seems to be dwelt upon with considerable complacency, is, "that chyle, however diversified the materials may be out of which it is formed, whether animal or vegetable, has essentially an identity of nature. This "fact of the perfect and uniform constitution of chyle seems, at once," we are told, "to put down the hypothesis" that any crude substances are ever taken into the circulation. Now, against this argument we would interpose one or two objections. It does not appear, in the first place, to be really the fact, that the chyle, whether formed from animal or vegetable food, possesses this "perfect uniformity and identity of constitution," for which our author contends. So far from this, the very reverse has been completely established by one of the best chemists now living. Dr. Marcet, in his analysis of chyle,* informs us that the chyle from vegetable food yields, by analysis, about three times as much charcoal as that from animal food; that the chyle from animal food generally begins to putrefy in three or four days, whilst that from vegetable food can be kept for weeks, or even months, without undergoing putrefaction; that the chyle from animal food is always milky, and on standing, an unctuous white creamy substance collects on the surface; its coagulum is opaque, and has a pink hue; the chyle from vegetable food is commonly transparent, or nearly so, like common serum; its coagulum is nearly colourless, like an oyster, and no creamy substance rises to the surface. This statement certainly exhibits several very striking points of difference between the chyle formed from animal food, and that formed from vegetables; and it unquestionably does away the broad position of our author concerning the "perfect uniformity" of chyle. Should Dr. Chapman endeavour to shield himself from the force of this representation, by urging that the difference pointed out in the analysis of Dr. Marcet is merely a variation in the proportions of the same elementary ingredients, we will refer him to his own work for a summary refutation of such a suggestion, when he tells us, "that experiments have fully demonstrated, that articles widely discrepant in their general nature, as aliment and medicines, the most salutary food, and the rankest poison, exhibit, on analysis, nearly the same results. This indeed holds so generally true, that the virus of the viper, and the mildest mucilage, the poisonous prussic acid, and the nutritive flesh of animals, constitute no exception. Decomposed into their elementary principles, they are essentially the same." vol. i. p. 42. If, therefore, a variation in the proportions and combinations of its elementary ingredients do not constitute a real difference in the chyle, then it follows from Dr. Chapman's own showing, that there is no difference between "aliments and medicines"—between "the most salutary food and the rankest poison"—between "the virus of the viper, and the mildest mucilage"—or between "the poisonous prussic acid, and the nutritive flesh of animals."

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* Medico-Chirurgical Transactions, vol. vi. p. 630.

A second objection which we have to make against this doctrine of the "perfect and uniform identity" of chyle, is, that it is positively contradicted by facts too well attested to be set aside. By Musgrave, Fordyce, and others, it has long since been shown, that certain articles, taken into the stomach with the food, will impart their peculiar colour to the chyle, and must therefore have become incorporated with it. By Dr. Chapman this is denied, on the ground of experiments made by himself and others, in the University of Pennsylvania, although we think he is very far from being borne out in his conclusions, even by his own statement. "None of the preparations of iron," he tells us, "of copper, of lead, nor the colouring matter of indigo, of madder, or of rhubarb, can be traced even as far as the chyle. Being introduced into the small intestines of dogs, *these several articles were observed to be rapidly taken up by the lacteals*, the coloured ones losing their tints on passing on, and, in every instance, so completely were their properties obliterated, as not at all to be cognizable by any chemical tests in the contents of the thoracic duct."—vol. i. p. 68.

Dr. Chapman, we presume, will not pretend that the lacteals contain any other fluid than the *chyle*, and if so, he upsets his whole argument by the admissions which he makes in the paragraph just quoted. The fact that the chyle, when it reached the thoracic duct, had lost the colour imparted to it by the various substances mentioned in these experiments, may be explained in a very simple and satisfactory manner, by recollecting that as the indigo or the madder passed on from the lacteals to the thoracic duct, it became successively diffused through a larger quantity of chyle, and therefore it was a very obvious and necessary consequence that the colour communicated to the chyle should become constantly fainter, until finally it was completely lost. A satisfactory illustration of what we mean is at the command of every person. By dissolving a portion of indigo in a small quantity of milk or water, it will be found to give to these fluids a very deep tinge. By adding successively however of the milk or water, the colour will gradually fade, until at last it will be scarcely perceptible. Precisely in this way do we account for the fact mentioned by Dr. Chapman.

We have thus briefly stated what appear to us unanswerable objections to our author's argument concerning the "uniform identity" of the chyle.

A second argument of Dr. Chapman's against the doctrine that medicinal substances are taken into the circulation, is, that if such were the case, it would be attended with the most fatal effects upon the system, inasmuch as the mildest fluids cannot be injected into the blood vessels without destroying life. That his reasoning may be perfectly understood, we shall quote his own language: "It must be acknowledged," says he, "that no substance, in its active state, does reach the circulation, since it is shown, that a small portion even of the mildest fluid, as milk or mucilage, oil or pus, cannot be injected into the blood vessels, without occasioning the most fatal consequences. Twenty-two years ago, in conjunction with my friend, the late Dr. George Lee, then resident in the Pennsylvania Hospital, I instituted a series of experiments to this purpose. All the articles enumerated above were tried in succession, together with some others of an acrid and stimulating nature, on dogs and cats, the animals selected on the occasion. But diversified as these substances are, we could discern no material difference in their effects, the whole seeming to act merely as extraneous matter in *error loci*, producing, at first, great distress to the animal, as was indicated by its movements and cries, followed by difficult panting, respiration, vomiting, and purging, nervous tremors, convulsions, and death. Experiments very analogous to the preceding have recently, I understand, been made by Professor Caldwell, and with confirmatory results. That the late inquiries of Sir Everard Home and others lead to a different conclusion, I am aware. Confiding, however, in the accuracy of our own observations, I must, in the present state of the question, still maintain, without the slightest qualification, the position I have assumed."—vol. i. p. 64, 65.

Before we proceed to the examination of this argument, we cannot help expressing our regret that the experiments to which Dr. Chapman alludes, as having been performed in Philadelphia, should differ so widely from those conducted in other parts of the world, because it serves not merely to shake our confidence in men upon whom we have been in the habit of looking as able and accurate experimentalists, but also to weaken our faith in the accuracy of the experiments of Dr. Chapman himself. For assuredly, if Sir Everard Home, Majendie, and others, who have devoted their lives to these investigations, are not to be depended upon, we may be forgiven, we think, for cherishing a little scepticism concerning the infallibility of Dr. Chapman or Dr. Caldwell. Without intending to impeach in the least the correctness of our author's experiments, it is impossible for us to get rid of the mass of opposite testimony, which we find recorded, on authority too respectable to be despised. Independent of those reported by Home and Majendie, there are many other facts to prove, that medicines introduced into the blood-vessels may prove not merely innocuous, but even salutary. By referring to some of the early volumes of the Transactions of the Royal Society of London, Dr. Chapman will find several cases related, in which patients were cured by medicines thus introduced into the system. Admitting, however, for the sake of argument, the objection of Dr. Chapman to its fullest extent, we cannot perceive that it has much force. Supposing that medicinal substances, when injected into the blood-vessels, do produce such deleterious effects, it does not seem of necessity to follow, that similar consequences should result from them, when introduced into the circulation, through the medium of the stomach and lacteals. On the contrary, the difference in the two modes of introduction, seems so great as really to destroy all analogy between them. In proof of this, it need only to be mentioned, that the chyle, the natural fluid circulating through the lacteals, will, if injected into any of the blood-vessels, produce effects precisely similar, and equally injurious with any of the foreign substances alluded to by Dr. Chapman. Hence, the conclusion is inevitable, that substances may pass innocently into the system through *the lacteals* which would be succeeded by the most deleterious consequences if introduced directly into *the blood.* And if so, our author's argument falls at once to the ground. We might here enlarge, for the purpose of showing how wide a difference there is, in the *manner*, in which substances enter the circulation in the two cases, but we think this must be so obvious to every person of reflection, that we shall remain silent on the subject, and pass on to the next argument of our author.

A *third* objection of Dr. Chapman is couched in the following language: "Conceding, however, to the humoral pathologists all that their doctrine demands, still *insuperable difficulties* remain in the way of its adoption, to account for the operation of medicines. Not to dwell tediously on the subject, I shall content myself, at present, with little more than mentioning, that we are not at all informed by it, why our remedies, after mixing with the blood, should be directed to one organ in preference to another, as mercury to the salivary glands, or how indeed they operate at all."—vol. i. p. 65. Now, we cannot see what Dr. Chapman gains by this objection, for it applies with quite as much force to his own theory. He tells us indeed that medicines are directed to particular parts of the system "through the medium of sympathy;" at the same time confessing that "of the *manner* in which impressions are extended, as well as of the *cause* of the more intimate consent of parts, we are not, perhaps, accurately informed." If, therefore, Dr. Chapman's own theory is confessedly inadequate to the explanation of this phenomenon, we have yet to learn by what rule of sound logic he is justified in raising this objection against another theory.

After all, we cannot see any of those "insuperable difficulties," in explaining this subject, which appear to weigh so much upon the mind of Dr. Chapman. That every organ in the body has its peculiar and appropriate stimulus, by which it is excited into action, is so universally admitted as to require no process of reasoning to establish it. Our author concedes it, and indeed it forms the basis of his classification. Hence it follows that certain medicines, when introduced into the system, act upon particular organs, leaving the rest more or less unaffected. If this be so, then there can be little difficulty, we think, in conceiving that a substance dissolved in the blood may circulate through the system, without producing any particular effects until it reaches the organ upon which, from its peculiar properties, it is designed to operate. The reason *why* a medicine acts upon one organ, in preference to all the other organs of the body, why jalap, for instance, operates upon the intestines and not upon the brain and lungs, we can no more explain than we can the reason *why* the planets are kept revolving in their orbits. If we are told that the movements of the planets are the result of attraction, so we may say that the determination of medicines to certain organs is occasioned by a similar kind of attraction. This however explains nothing, and we must after all be content with the broad *fact*, that such phenomena do occur, and that they are governed by certain laws; but the *cause why* they occur must ever remain concealed. Upon this ground, we think Dr. Chapman is asking too much to require that we should assign the cause "*why* remedies, after mixing with the blood, should be directed to one organ in preference to another." All that the most rigid disputant can reasonably demand or expect from us, is to prove, in the first place, that medicines are actually taken up into the circulation; and in the second place, that they afterwards act upon particular parts of the system. The latter is admitted on all hands, and the former rests on proofs too solid to be shaken.

A *fourth* objection of our author's is, that "by the *medication* of the blood, were it possible, as is contended for, we must in all instances do harm. The whole mass of circulating fluids is equally charged in this case with the medicinal substance, and therefore, while an action is going on in a diseased organ, which may be salutary as to it, every sound part of the system becomes subjected to a similar impression, which could not fail to disturb the order of health, and create morbid derangements."—vol. i. p. 65, 66.

All this is purely hypothetical. It is founded upon a supposition entirely erroneous, which is, that a medicine cannot act upon one part of the system to the exclusion of the rest. In what we have already urged, we have in fact so fully replied to this argument, that we deem it unnecessary to enter into any recapitulation.

We have thus considered all the material objections of Dr. Chapman, against what he chooses to call "a relick of the humoral pathology." Aware of the strong support which this "relick" receives from indisputable facts, he endeavours to explain them away in a manner equally singular and original. That we may have before us the full scope of his reasoning on this point, we shall quote him at length. " That some of the properties of certain articles are displayed," says he, " in the secretions and excretions, I am not disposed to deny. But it does not hence follow, that these substances entered the circulation in their primitive state. Directly the reverse, indeed, seems to be proved, as no one of them can be detected in the serum of the blood. To me it is clear, that the process of assimilation, as performed either by the chylopoietic viscera, or by any part of the absorbent apparatus, completely *decomposes* all substances ; and, however discrepant in their properties, reduces them to a homogeneous fluid fitted for the purpose of nutrition. But, when thrown into the secretions or excretions, being removed beyond the control of the vital energies, chemical affinities are sometimes again brought into play, by which these substances are in part, or wholly regenerated. No slender support is given to this hypothesis by the well-known fact, that matters are found in such positions, which had not previously existed, in any cognizable state, in the blood. Thus, certain articles can only be detected in certain fluids, as the odour of garlic in milk, of asparagus in urine, of sulphur in the perspiration, and the colouring principle of madder is to be traced in no part of the solids, except the bones, and their immediate appendages, the cartilages. Did these articles pre-exist in the blood, instead of being regenerated in some such manner as I stated, ought they not to be thrown out indiscriminately by all the enunctories ?"—vol. i. p. 63, 64.

The most sturdy of the humoral pathologists, we think, could not wish Dr. Chapman to concede more than he has actually done, in the foregoing paragraph. He admits that certain substances are carried into the circulation, and that they display their peculiar properties in the secretions and excretions. But says he, they do not enter the circulation, " in their primitive state ;" they are reduced by the " process of assimilation," and thus enter into the circulation, and when thrown into the secretions and excretions, by the "play of chemical affinities, they are again, wholly or in part, regenerated." Let us analyse this explanation, and see what it amounts to. We are told that sulphur, taken internally, exhibits itself in the perspiration. According to Dr. Chapman's solution of this fact, the sulphur, before it is taken into the circulation, is reduced in some way, so as to deprive it of all its characteristic properties, and when thrown out upon the skin by the "play of chemical affinities," is again regenerated and exhibited in the form of sulphur. To this explanation, objections of so strong and obvious a character present themselves, as to render it wholly inadmissible. We know nothing of sulphur, but as a *simple* substance, and Dr. Chapman ought to prove that it is *capable* of decomposition, before his explanation will hold good. Let us take another illustration. Nitrate of silver, taken internally, frequently produces permanent discolouration of the skin. Cases of this kind are on record,* and we presume will not be disputed. Dr. Chapman would argue, that the nitrate of silver is first decomposed into its elementary ingredients, and afterwards regenerated on the surface. The nitrate of silver then would be decomposed into nitric acid, and an oxide of silver: the nitric acid again would then be decomposed into oxygen and azote; and the oxide of silver into oxygen and the pure metal. Further than this it is impossible to decompose them. Here, then, according to Dr. Chapman's own explanation, we have silver, a large quantity of oxygen, and azote, which must all enter into the blood in their separate and uncompounded state, or else it is impossible for the nitrate of silver to be formed afterwards upon the surface. It is evident, that this solution multiplies the difficulties which it was intended to remove. According to it, instead of *one*, we have actually *three* foreign substances entering into the blood. Dr. Chapman certainly could not have been aware of the consequences of his own doctrine, or he never would have urged it so boldly. Again; the colouring principle of madder, which is found in the bones, Dr. Chapman tells us, is decomposed by the chylopoietic viscera; and again regenerated in the places where it is detected. Now, before we admit this, it must be proved that this colouring principle is capable of being decomposed. This, we think, would be rather a difficult undertaking. Admitting, however, that it might be decomposed, it would be no less difficult to show that it could afterwards be regenerated. Vegetables undergo *destructive* decomposition, and can never be regenerated by the art of the chemist, or by any play of chemical affinities. In every point of view, therefore, the explanation of Dr. Chapman is wholly inadmissible; being inconsistent with itself, and contradictory to known and acknowledged principles.

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* See Medico-Chirurgical Transactions of London.

But, says our author, " did these articles pre-exist in the blood, instead of being regenerated in some such manner as I have stated, ought they not to be thrown out indiscriminately by all the enunctories?" We answer no. Such a consequence no more follows than that all the glands of the system should secrete precisely the same kind of fluid. With just as much propriety might it be asked, why the liver does not secrete milk, or why the stomach does not secrete urine. The truth is, every secreting organ is destined to separate from the blood a *peculiar* substance, and hence it is perfectly plain why foreign matters, circulating in the blood, are not " thrown out indiscriminately upon all the enunctories."

Having given his objections to the doctrine that medicines ever enter the circulation, our author proceeds to explain his own theory of the *modus operandi* of medicines.

" It results, on the whole, from what I have said, that we are to reject the fluids altogether in our inquiries relative to the operations of medicines; because in addition to the reasons already stated for doing so, we have in that law of the animal economy, termed sympathy, or consent of parts, a solution of the problem, which comports infinitely better with the existing state of our knowledge.

Conformably to the theory I have adopted, whenever a medicinal substance is applied to a susceptible portion of the body, externally or internally, an action is excited, which is extended more or less, according to the diffusibility of the properties of the substance, or the degree of sympathetic connexion, which the part may maintain with the body generally. Thus a set of actions is raised, every one of which is precisely similar, provided they are confined to the same system, by which is to be understood parts of an identity of structure. If, however, the chain runs into other systems, it loses its homogeneous character, the actions being modified by the peculiar organization of the parts in which they may take place. These are principles of universal application. In every case, whether it respects the operation of remedies, or the production of disease, the spot primarily acted upon, is a point from which is diffused the radiated impressions."—vol. i. p. 70, 71.

It may easily be gathered from the sentiments which we have already expressed, that we do by no means subscribe to this theory. Like most sweeping generalizations, it is altogether too exclusive, and fails to account for the operation of a very large portion of medicinal substances, which seem to act upon a principle entirely different. We want no better proof of the inadequacy of this theory, than Dr. Chapman's own work. Take for instance, what he says concerning the Oil of Turpentine. In treating of the virtues of this article in correcting the lithic diathesis, and relieving nephritic pains, he adds, "on what principle it operates is not very intelligible, though it would seem that its appearance in the bladder is necessary to its success. I am told by Dr. Physick, to whom I owe much of the information which I possess on the subject of this article, that whenever it has failed with him, the *violet odour* was wanting in the urine."—vol. i. p. 343.

It is very plain that Dr. Chapman here abandons his own theory; for if the doctrine of sympathy could explain the operation of the turpentine, why is he forced to confess that the "principle on which it operates is not very intelligible?" Nothing can be clearer than that sympathy, at least as explained by our author, has nothing to do in this case. The fact speaks for itself. The turpentine must be taken into the circulation, and afterwards act directly upon the urinary organs, or else the *violet odour* could not be detected in the urine. Here, then, is at least one clear and pointed exception to Dr. Chapman's theory.

Let us take another illustration. In discussing the general action of "Lithontriptics and Antilithics," we are told that the possibility of dissolving a stone in the bladder by a course of medicine rests upon two grounds, one of which is the following, viz. "that some of these solvents do reach the urinary bladder, without any, or, at least, a material change being wrought in their properties, so that, when coming in contact with the stone, there might be a play of chemical affinities, and hence a decomposition of the calculous body."—vol. i. p. 322. Now, here is an unqualified admission, of not a *single* medicine, but a *whole class* of medicines acting upon principles purely *chemical*. In so far, therefore, as this class of remedies is concerned, it cuts short the doctrine of sympathy, and proves beyond a doubt, that the unlimited manner in which that doctrine is maintained by Dr. Chapman is wholly untenable. No "sympathetic actions," nor "associated motions," nor "radiated impressions diffused from a point," can ever dissolve a stone in the bladder, or account for the varied appearance of the urine, resulting from the use of internal remedies.

Another exception to our author's theory may be found in the list of Anthelmintic medicines, a large portion of which are admitted to act by their poisonous properties upon the animal. But we will not push this subject any further. Enough has been adduced to prove, that the doctrine of sympathy can only be admitted with such large qualifications and exceptions, as to overturn completely the theory of our author, that medicines uniformly and universally act upon this principle alone. We hope we shall not be misunderstood. In explaining the operation of medicines, we do by no means deny altogether the agency of sympathy, or some principle very similar to it. To contend, that all medicines act through the medium of the circulation would, in our opinion, be not less contradictory to reason and fact, than the doctrine which we are combating. We do not believe in this wonderful simplicity of nature, which has been so favourite an argument with every theorist who has attempted to explain the intricacies of a whole science upon *one* general principle. Dr. Chapman, for instance, tells us, that "to multiply causes superfluously is against one of the fundamental rules of philosophising, and is not less repugnant to the general course of nature, whose means are proverbially distinguished by great simplicity and uniformity." This is true indeed; but then it is equally unphilosophical, not to assign causes sufficient to explain known and acknowledged phenomena; and as it regards the present case, it rests with Dr. Chapman to show that by calling in the aid of the circulation we multiply causes unnecessarily. An excessive fondness for simplification has been the bane of medical science. It was this which led Brown to maintain the absurd doctrine that all medicines were stimulants, the only difference between them being in the force and rapidity with which they exercised their stimulating powers. It was this which prompted Dr. Rush to defend the strange notion of the unity of disease, and it is this which induces Dr. Chapman to contend for the exclusive agency of sympathy, in explaining the operation of medicines. Wanting, as they all do, the broad and substantial basis of truth and philosophy upon which to rest, it is impossible that their influence should be other than transient and ephemeral. The theory of Brown has long since been abandoned. The unity of disease, notwithstanding the zeal, and talents, and eloquence with which it was defended, was falling by its own weight, even before the death of its illustrious author. And it requires not the spirit of prophecy to predict that a similar fate awaits the theory of Dr. Chapman.

But we have dwelt long enough upon this part of our author's work, and we hasten to a brief notice of some other portions of it.

In treating of the general effects of medicines, and after telling us that "he holds all medicines to be stimulants," Dr. Chapman takes occasion to give us a definition of the terms *stimulant*, and *sedative*, which does not appear quite so satisfactory as we could have wished. "As, whatever produces," says he, "any positive impression on the living system, does it commonly by an incitant power, there can be no doubt that, in a very strict sense, most medicinal substances are stimulants. But the effect thus created may be inferior to the natural degree of excitement, and much less than that of disease. When this happens, as lessening action, the article may, with propriety, be demonstrated, and in a practical view considered, sedative, in contradistinction to our more energetic remedies."

This language is certainly not very clear; but if we do not strangely misconceive it, it goes evidently to support the belief of the existence of medicines which are directly sedative. That Dr. Chapman did not mean to convey this idea, we take for granted, from his having stated just before that he considered all medicines to be stimulants. We object merely to the want of precision in the language, which is the more to be regretted, because it is a subject of which Dr. Chapman himself tells us that "the discussion, which has so long been maintained on this point, is really little else than a dispute about words."

It is impossible for us to enter into a particular review of every portion of Dr. Chapman's work, and indeed we consider this the more unnecessary because it is, or ought to be, in the hands of every American physician. The chapters on "the practical application of Emetics and Cathartics," we consider as among the most valuable and interesting portions of it. They are rich in suggestions of practical import, and we would especially recommend them to the notice, not merely of the student of medicine, but of the practitioner.

On the subject of Emmenagogues, we are presented with a discourse equally interesting and instructive, although the theoretical notions which it inculcates in favour of the menstrual discharge being a *secretion*, are liable to many serious, and, as we think, unanswerable objections.

Under the head of Opium, Dr. Chapman speaks of the use of this article in catarrh, and gives it the following recommendation: "*Novel* as the remedy I have suggested in catarrh may seem, it is not without the support of experience. I have tried it an hundred times on myself, and still oftener with my patients, so that I can hardly be deceived. Taken on going to bed, which it should be, it soon excites an universal and equable glow over the system, attended with little or no perspiration, and I am not aware that any advantage is gained by uniting with it a diaphoretic. It probably operates here simply as a diffusable stimulant, overcoming, by its superior powers, the feeble action of the incipient stage of the disease. Delayed till the attack is confirmed, opium becomes mischievous, and we can only hope to subdue it, if violent, by calling into requisition the depletory and antiphlogistic measures."—vol. ii. p. 209.

We have extracted the foregoing passage, because it touches upon a subject extremely interesting; but more especially because it would seem, from the language of our author, that he intended to convey the idea that the practice here recommended is original. Dr. Chapman may not have been indebted to any one for the suggestion; but it is not less certain, that the very same practice was recommended, upwards of forty years ago, by Mr. Mudge, in his treatise on "A radical and expeditious cure for a recent catarrhous cough,"* &c. &c. It must be admitted, that in addition to the opium, Mudge considered the inhaling of warm steams into the lungs as essential to the cure. As regards the opium, however, the practice is radically the same, and he subjoins a caution similar to that given by Dr. Chapman, that it is to be used only in the *commencement* of the disorder.

We are aware that our remarks have already extended themselves too far, and we shall bring them to a close, after having corrected an error into which our author has inadvertently fallen, in his discussion on the subject of Iron. "Experiments exceedingly numerous," says he, "and well contrived, which were originally made by Dr. Edward Wright, and subsequently repeated by Dr. Hodge, a graduate of this University, under every circumstance of care and attention, demonstrate, that *none of the preparations of this metal* can be detected even in the chyle."—vol. ii. p. 461. Of Dr. Hodge's Experiments, we know nothing. But the statement concerning Dr. Wright, is not quite correct. In the eleventh volume of the abridged Transactions of the Royal Society of London, we find an account of an experiment made by Edward Wright, M. D. from which it appeared that salt of steel does not enter the lacteal vessels. In the same paper, however, Dr. Wright admits that the accurate experiments of Menghini, in the memoirs of the Bononian Academy, sufficiently prove, that the ore and filings of iron, finely levigated, enter the blood in considerable quantity, as does also the crocus, calx, or earthy part of the metal, though in less quantity than the two former, which were found to act with a violent stimulus on the vessels, and to have dissolved and broken the crasis of the blood of different animals that had used them, for some weeks, in large doses, mixed with their ordinary foods. He moreover refers to the experiments of Lister and Musgrave, to prove that particles much grosser than those of white chyle, provided they be not astringent or very acid, are conveyed by the lacteals. The reason which he assigns for the salt of steel not entering the lacteals, is its great astringency, by which their mouths are so constricted as to prevent them from taking it up.

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* Duncan's Medical Commentaries, vol. iv. p. 150. Am. ed.

To some of our readers it may appear strange that after the high encomiums which we passed upon the Therapeutics of Dr. Chapman, at the commencement of this article, we should have devoted almost the whole of it to an exposition of what we conceived to be some of its errors and deficiencies. A very little reflection will, however, we think, render the cause perfectly obvious. The errors of men of genius are peculiarly dangerous. Coming to us, as they do, recommended by all the fascinations of taste and eloquence, their influence is proportionably extensive and commanding. Over youthful and inexperienced minds, especially, they bear a sway which not unfrequently sets reason herself at defiance. It is for this cause that we have thought it our duty to interpose some objections to the reasonings and theories of our author. Entertaining, as we do, the very highest respect for Dr. Chapman, it would be to us a source of the most unfeigned regret, should a single word that may have dropt from us, be construed as savouring of disrespect or hostility. So far from this being the case, circumstances, which it would not be proper to mention in this place, have inspired us with sentiments of the kindliest regard, and it is with no false or deceitful pen that we wish him all the prosperity to which his character, his talents, and his labours in the cause of science so pre-eminently entitle him. B.

ART. II. MEDICAL DISSERTATIONS ON HEMOPTYSIS, *or the Spitting of Blood, and on* SUPPURATION, *which obtained the Boylston premiums for the years* 1818 *and* 1820. By JOHN WARE, M. D. Fellow of the Massachusetts Medical Society. Boston: Cummings & Hilliard, 1820. pp. 95.

THE subjects treated of in these Dissertations are of a very interesting nature, and, generally speaking, are managed in a way very creditable to their author. This, however, is not our only, nor in fact our principal reason for noticing them. We wish especially to recommend them to public notice, as the offspring of a patronage extended to science which has hitherto been very uncommon in this country, but which we hope, ere long, to see more extensively imitated. There is nothing of which science in the United States stands so much in need as encouragement and patronage. We have talent, and ingenuity, and industry, and activity in abundance, and were appropriate inducements held forth, it would be found that the products of American intellect would very speedily rival those of the most cultivated nations of the old world. Entertaining such opinions, we should not be true to ourselves, did we not applaud every act of private as well as public liberality that may be directed to the evolution of *native* talent, and the reward of *native* industry. On this account we feel pleasure in recording upon our pages, the name of WARD NICHOLAS BOYLSTON, of Boston, a liberal patron, to whom medical science will long feel itself much indebted.

This gentleman, several years ago, made an appropriation of a sum of money, the interest of which was to be distributed, in the form of prizes, to the authors of the best dissertations on subjects connected with medicine. This fund, we understand, is under the superintendance of Harvard University, and for the purpose of examining the dissertations and adjudging the premiums, a board of learned and respectable gentlemen has been constituted. Of the dissertations which have in successive years gained this prize, several have already been published, which are highly honorable to their respective authors. Dr. Ware, it appears, has been so fortunate as to receive more than one of these prizes. One of the dissertations before us gained the prize in 1818, and the other in 1820. The first of these, namely, that on the subject of Hemoptysis, is we think the best of the two, and we shall accordingly present our readers with a brief abstract of the doctrines which it inculcates. After some general observations on the peculiar liability of the lungs to disease, Dr. Ware takes up the consideration of the proximate cause of Hemoptysis, and discusses the different theories which have prevailed on this subject. The most popular opinion is that which attributes it to a rupture of the pulmonary vessels; the technical name given to this kind of hemorrhage is *rexis*. Another theory ascribes it to an erosion of the vessels from some existing acrimony of the fluids; this has received the name of *diabrosis*. A third considers it to result from a transudation of the blood through the membranes of which the vessels are composed; this has been called *diapedesis*. While, by a fourth theory it is presumed to originate from *anastomosis*, or a simple dilation of the orifices of the extreme vessels.

Rejecting the three first of these opinions as altogether inadequate to explain the phenomena attendant upon hemorrhage from the lungs, our author adopts the last as agreeing best with "both physiological and pathological observations." As observed by our author, this doctrine is not new. It has received the sanction and support of many eminent physiologists and physicians. Among these the most distinguished is Bichat, and the ingenious reasonings of this author on the subject are given at length by Dr. Ware; in addition to which he has furnished some observations of his own, which go to support the doctrine for which he contends. It is not our intention to go into an examination of the merits of this theory. Our limits would not permit us, even if we felt so disposed. We shall merely state, that although some very obvious objections may be urged against it, yet, upon the whole, it is supported by a train of facts and arguments the force of which it would not be very easy to destroy.

Our author next proceeds to consider the *remote causes* of Hemoptysis. A plethoric state of the system, he thinks, has less to do in the production of this disease than has commonly been supposed. "Hemorrhage," he admits, "is doubtless sometimes occasioned by plethora, but not commonly; and hemoptysis seldom." It seems to depend more upon a plethora of certain organs, than of the system generally. "An abundance of blood in the body will not be apt to produce hemorrhages if it be equally distributed and properly balanced. It is only when the balance is destroyed that injury ensues; and although this is perhaps more easily done in a plethoric habit, yet it may take place under any circumstances, and produce the same effects."

The predisposition to hemoptysis Dr. Ware considers as pretty much the same with a predisposition to phthisis, and when such predisposition exists, hemorrhage from the lungs may be excited by any cause which increases the circulation in the system generally, or in the lungs in particular. Among the former are enumerated exposure to great external heat, as in manufactories, glass houses, &c. all violent straining, or exercise of the muscles, as in running, jumping, and walking for a long time. Among the latter, public speaking or singing, shouting loudly, playing on wind instruments. The suppression of accustomed discharges, as the catamenia, piles, &c. are also mentioned among the causes of this complaint.

Our author next details the *symptoms* of this disease.

> "In whatever manner hemoptysis is occasioned, it generally comes on with very nearly the same symptoms. The discharge of blood is preceded by a weariness and lassitude affecting the organs of motion, a shivering sensation and constriction of the skin—by a coldness of the extremities—a sense of weight or anxiety about the precordia—a slight degree of pain in some part of the chest—a sensation of heat or of burning under the sternum—and by a shortness and difficulty of breathing increased by moderate exercise. These symptoms may have existed for a longer or shorter time before the hemorrhage occurs, but sometimes it comes on suddenly without any such warning, the flow of the blood being the first symptom observed. This is said frequently to be immediately preceded by a saltish or brackish taste in the mouth, the cause of which, if it be any thing more than the fancy of the patient, is probably owing to a sympathy of the organ of taste with the diseased part. A slight tickling is felt at the top of the windpipe, which excites a coughing to relieve it. This brings up the blood from the lungs, of a florid, arterial colour, and of a frothy appearance, at first in a small quantity, but the irritation continuing, more is brought up in the same way, with a rattling noise in the windpipe, as of air passing through a fluid. In some instances, only a few mouthfuls are thrown up, before the hemorrhage ceases; in others, a slight spitting continues for several days together, and sometimes, especially when it has repeatedly occurred, it is in great quantity, and the blood is thrown out so copiously, that the patient seems rather to be vomiting than coughing it up. It is seldom that the hemorrhage is so abundant as to cause death either by suffocation or exhaustion. It generally ceases spontaneously, when sufficient blood has been poured out to relieve the vessels of the lungs; or it is checked by the use of remedies."—pp. 22, 23.

In the *treatment* of Hemoptysis, blood-letting is recommended as the first and most important remedy. We are happy to find that Dr. Ware does not restrict the use of blood-letting to vigorous constitutions, or plethoric habits. His opinions on this subject accord so fully with our own, that we cannot help extracting them.

"Writers in general," says he, "have recommended to confine its use to strong and plethoric habits; but I do not see why this should be thought to make so much difference. Do we find that those of weak and slender habits are more debilitated by bleeding than those who are stronger? At any rate, are the bad consequences of the operation so grave and dangerous as those which ensue from hemoptysis? And does it not as often prevent these consequences from taking place in those who seem to have no blood to spare, as in those who are full of it. If they are not able to bear the operation of the remedy, they are still less able to support the disease. It may be said that our opinions are derived from theoretical views with respect to the nature of hemorrhage; but is it not theory which restricts bleeding to those who are in vigorous health? We ought undoubtedly to have some regard to the power of the system to repair the loss of blood: There are certainly cases in which this remedy is improper; but in any subjects, with respect to whom it would be adopted, were they attacked with inflammation of the lungs, there seems to be no danger in resorting to it in hemoptysis."—vol. i. p. 29.

This is sound and wholesome doctrine, and if it had been acted upon more generally than we fear it has been, might have saved many a valuable life to the community.

After blood letting, *emetics* are next recommended, and our author appears to set a high value upon them. "Of their great importance," he says, "there is no doubt, and they seem to be called for in the majority of cases. They may be useful in two ways: first, when the stomach is deranged either from previous disorder, or from the attack itself, by putting it in good order; for the system never acts to advantage under any difficulty if this is not done; and secondly, by the sympathetic operation which vomiting has upon the capillary circulation of every organ, particularly of the lungs and skin."

The state of the *bowels* is particularly to be attended to. As *cathartics*, the neutral salts, as operating with least irritation, are specially advised. In cases where the pulmonary affection is combined with some disorder of the digestive organs, it may be necessary to give the preference to other substances.

The use of *astringents*, in this disease, Dr. Ware tells us, is "indicated principally in those cases where bleeding and the other measures employed do not succeed; where any particular circumstance prevents their employment; or where the effusion of blood is so sudden and profuse as to cause the immediate loss of life from hemorrhage." The mineral astringents are the most powerful, and are generally used. Among these, the best are the acetate of lead, the sulphates of zinc and of copper, the super sulphate of alumina and potash, and sulphuric acid.

Exciting counter-irritation by means of *blisters*, *setons*, or *issues*, is highly commended in cases where the spitting of blood continues, at short intervals, for some time, accompanied by a slight dry cough, pain in the side, and across the chest, and shortness of breath. In these cases Dr. Ware thinks counter-irritation, by the means already mentioned, "becomes a remedy of great power, and is perhaps the most certain we can employ."

Of the use of Digitalis in Hemoptysis, our author speaks in the following terms:

> "Digitalis is an article well worthy of a trial, in cases which do not yield to the measures already recommended. Though far from doing good in a majority of instances, it is in some of such evident advantage, that one would hardly feel justified in omitting its employment where the usual remedies had failed to check the hemorrhage, or where serious symptoms of pulmonary disease continued, after the violence of the first attack has been subdued. I know of no particular circumstances by which we are enabled to distinguish the cases in which this remedy will be of service, though such a power of discrimination would be of great value. On subjects, to all appearance affected in the same way, it produces entirely different effects, without affording any clue to the principle on which this difference is founded. It is possible, that it may

have some connexion with the very marked and powerful influence possessed by Digitalis over the lymphatic system, in Dropsy." p. 33.

One of the best parts of Dr. Ware's Essay is that which relates to the liability of public speakers, and especially the clergy, to hemoptysis. His observations on this point are so judicious, and furnish so favourable a specimen of his manner of writing, that we shall quote him at some length. The importance of the subject, we hope, will excuse us for so doing.

"I allude to public speakers, especially those of the clerical profession. The many and valuable lives which have among us fallen apparently a sacrifice to the duties of this profession, have given to the subject a painful degree of interest. So little can be done with the hope of restoring those who have once suffered from hemoptysis with a tendency to phthisis, that it seems of the first consequence to ascertain what the exact circumstances are, in the life, habits, and duties of a clergyman, which render him peculiarly liable to such an attack, and to the consequences it brings in its train.

"The most obvious cause of this liability to pulmonary disease in clergymen, is the great and long continued exercise of the lungs, required in the performance of public worship. To this, as the peculiar duty of the profession, our attention is apt to be principally and almost exclusively devoted; we are too ready to consider it as a sufficient cause in itself, and to avoid examining the influence of other circumstances. It is not found that members of other professions, who are in the habit of exerting their lungs, are more liable than the average of mankind to pulmonary disease. We do not hear of any extraordinary proportion of deaths from consumption among lawyers, public actors, public singers, &c. We must therefore look to other circumstances in the mode of speaking, the life and habits of our clergy, for the causes of this tendency among them to affections of the lungs, and derive from our investigation the best methods of counteracting their operation, and of avoiding the fatal issue to which they lead.

"It has usually been admitted by physiologists, that the exercise of any organ, if not carried to an extraordinary or unnatural degree, and if it be in a healthy state, contributes to increase its strength, and renders it capable of sustaining an increased degree of labour without injury. This, under some circumstances, is true of the lungs, though not to the same extent that it is with regard to the external voluntary organs. Public singers, and performers upon wind instruments, acquire by habit the power of exercising their lungs in a pretty violent manner for a length of time, to which at first they were totally inadequate. This partly depends, without doubt, upon the facility which they acquire of producing the same effect with a less exertion of strength ; but it cannot be entirely attributed to this circumstance. A few moments will exhaust a beginner upon wind instruments ; but almost any individual may gradually become able to play upon them for some hours without injury. Now, why is the case different with regard to the public speaking of clergymen, which certainly requires a much less violent exertion on the part of the lungs than either singing or playing on wind instruments ? The reason I believe to be this, that the duties of the profession are only occasional, and occur at too great intervals to allow the formation of a habit ; whilst at the same time they are sufficiently difficult to over exercise, fatigue, and exhaust the organ. They are from the first as long and as laborious as they will ever be, and there is no opportunity for that slow and gradual increase which enables one to acquire strength and facility of exertion. In order to strengthen the general habit, or any particular part, by exercise, it is not necessary to begin with any unusual or extraordinary efforts ; this is rather injurious, and defeats the object in view ; but it is necessary to keep up constantly some effect from the exercise ; to have the organ or the system under its regular and equable influence. It is only in this way that any benefit is to be derived from it in the treatment of disease. Occasional extraordinary exertion, carried to the point of fatigue, and then omitted until the fatigue is entirely removed, can only have the most injurious effect ; and it is exactly in this way that the lungs are exercised in preaching. They are wearied by the services of the Sabbath, and are then suffered to remain perfectly at rest through the interval of the week.

But there are other circumstances which give force and certainty to the operation of this cause, and which act by rendering the system liable to become diseased in any organ to which the exciting causes may happen to be applied. They produce a general state of predisposition to disorder, which becomes developed in the lungs, in consequence of the unnatural exertions which they are occasionally obliged to make, and would, under similar circumstances, be developed in any other organ. I refer to the habits of studying and living among the greater part of our literary men.

" They are, in the first place, accustomed to use but little exercise, and to devote a good deal of time to sedentary occupation. Their relaxation is too often of an inactive kind. Their exercise, if they are induced to attend to it at all, is seldom judiciously arranged; it is not regular and equal, but sometimes entirely omitted, and sometimes carried to excess. To the state of system produced by this inactivity, is added the effect of improper diet and improper quantity of food. In order to perfect health, it is necessary there should be a certain proportion between the demand for nourishment, and the supply provided. Other things being equal, the quantity of healthy exercise in the system determines the quantity of nourishment requisite for its necessities, and the quantity which the stomach can perfectly digest. If the exercise increase, the food must also increase; and *vice versa*. He, therefore, who leads a sedentary, and as it respects the body, an indolent life, does not require so much food, as one engaged in active occupation, and it is not necessary that it should be of so nutritious a quality. But the opposite course is in fact pursued by students. Their appetites, at least till they are become the subjects of disease, are not affected; or if so, are excited by delicacies or stimulants, and are indulged to an improper extent. The consequence is, that digestion is impeded; is performed for a while with difficulty and labour, then imperfectly in a greater or less degree, and, at last, almost entirely destroyed. This of course is the case with only a part of the individuals to whom I refer; the constitutions of perhaps a majority are sufficiently pliable to become accommodated to this manner of life sooner or later; but I apprehend I am correct in saying, that a great proportion of professional students in our vicinity, especially theological students, are affected, at one time or another, by some serious disorder of the digestive organs.—p. 35. 39.

Medical and Philosophical Intelligence.

DOMESTIC.

Removal of a portion of the inferior maxillary bone by an operation.—Dr. Mott of this city lately performed an important surgical operation, by which an entire section was made of nearly one half of the lower jaw-bone. The operation was had recourse to for the removal of a large Osteo-Sarcoma, which occupied so much of the mouth as to interfere very much with speech and mastication, and from its attachment to the velum palati and pressure upon the pharynx, impeded deglutition. The diseased portion of bone was removed from beyond the posterior angle to the side of the chin, the carotid artery having been previously secured by ligature. This operation was completely successful. The subject of it was a young lady, aged about 17 years, in other respects in excellent health, and of a good constitution. The particulars of this case will appear in a future number of this journal, accompanied with plates.

The use of Ergot in Uterine Hemorrhage.—We have just been informed by Dr. Hosack, that he has recently employed this medicine in a case of violent hemorrhage from the uterus, occurring after the cessation of the menses, with the most decided effect in restraining the discharges when all the ordinary astringents and other resources had been employed to no purpose. The medicine was exhibited in powder, in dosesof ten grains, three times a day. It produced a manifest action of the uterus, and an almost immediate check of the discharge. We are promised, for our next number, the particulars of this case, with additional observations on the use and abuse of this medicine.

Fever at St. Augustine, Florida.—We give the following particulars concerning the late fever which prevailed in St. Augustine:

they are derived from a source upon which reliance may be placed.

Nothing particularly remarkable occurred in the weather or the diseases incident to the place, prior to the commencement of the fever. About the middle of August, 1821, some cases of disease of a highly bilious nature made their appearance, and at the beginning of September they assumed a still more alarming and fatal character: the disorder was at its highest from the first to the fifteenth of October, during which period the greatest mortality prevailed. Towards the close of December the fever ceased.

The disorder attacked in an especial manner persons of a full and robust constitution; and without a single exception was confined to the new comers from more northern climates. It is stated that not a single individual from the West-Indies, or natives of the country, or any one who had previously suffered from yellow or malignant fever, became the victim to its influence. The fever was in a very striking degree characterized by all the leading symptoms of the yellow fever of the Northern states. It was marked with great prostration of the nervous system, and intellectual powers; it invaded suddenly, and generally terminated fatally within the second or sixth day from the attack; the peculiar yellow suffusion of the surface, as it is exhibited in yellow fever, was often observed, and that precordial anxiety, so uniformly noticed in the same complaint, was found to exist in a large majority of the cases of the fever of St. Augustine. In many cases the black vomit terminated the sufferings of the patient.

Forty or fifty deaths occurred principally among the new emigrants before the alarm became general. Eleven deaths was the greatest number that happened in any one day.

Of the inhabitants of St. Augustine, about two hundred is the full number that were exposed to the influence of the disease. Of this aggregate one hundred and forty were attacked, of which one hundred and thirty-two died. In these are included three blacks.

The disease also affected the troops in garrison, and forty deaths took place in a body of one hundred and twenty soldiers. The total number of deaths from this malignant fever was consequently one hundred and seventy-two.

In the treatment of this disorder, blood letting, which was had recourse to, but in a few cases, and at the commencement of the pestilence, invariably proved fatal. Calomel, when it could be prescribed so as to affect the general system and produce its action upon the mouth and gums, was servicable : but this salutary action was very rarely brought about, the patient dying before any evidence of mercurial excitation would be induced. In one instance where the salivary glands were affected it afforded no relief. The oil of turpentine was prescribed with advantage in several cases of deep gastric irritation.

Before the appearance of this fatal disorder, St. Augustine was noted for its uniform exemption from pestilential fever. A difference of opinion prevailed as to the origin of this malignant fever. Strong evidence has been brought forward that the disease was introduced from the Havanna, by the vessels *Alexander* and *Rapid* :—the first of which had lost her crew by yellow fever, and was brought in port by two passengers : the other had lost a part of her crew, and arrived at St. Augustine with persons who had died, and others sick of malignant fever.

American Flora.—We are happy to announce that a Flora of the Northern and Middle states is preparing for publication, by John Torrey, M. D. Vice-President of the Lyceum of Natural History, New-York, &c. This work will be comprised in two volumes, octavo, ornamented with plates, and will contain detailed descriptions, with synonyms, localities, &c. of all the plants hitherto discovered in the United States, north of Virginia. It will be published in numbers of 100 pages each. The first number will soon be put to press. The well-established character of the author for botanical and scientific attainments, affords ample assurance of the ability with which this work will be executed.

Report on the state of the New-York Hospital, for the year 1821.—The Report here inserted affords a full exposition of the state of one of our most efficient and important charities, and contains a brief but satisfactory account of the Lunatic Asylum, recently erected at Bloomingdale, by the munificence of the state, on a plan of unrivalled convenience and splendour.

The Governors of the New-York Hospital respectfully report, that in the course of the last year they have admitted 1658 patients, who, with 225 remaining on the 31st December, 1820, make 1883, who have received the benefit of the hospital during the year 1821. Of this number, 1543 have been cured, 111 relieved, 133 have been discharged at their own request, or that of their friends, and 23 as improper objects ; 22 have eloped or been discharged as disorderly, 166 have died, 52 have been transferred to the Bloomingdale Asylum, and 133 remained on the 31st December last. The above statement includes 27 women, who have been admitted into the Lying-in Ward, during the year 1821 ; who, with 3 that remained there on the 31st December last, make 30. Of these, 23 have been delivered and discharged in health, 4 have been discharged at their own request before delivery, and 3 remain in the Ward undelivered : 24 children were born in the Ward during the year, (there being one case of twins,) all of whom left the Hospital in health, having been previously vaccinated.

The above number also includes 35 male and 25 female maniac patients, admitted in the year 1821, and previous to the 27th July in that year, when they were transferred to the New Asylum at Bloomingdale, and making, together with 38 males and 23 females who remained in the Old Asylum on the 31st December, 1820, 121 maniac patients, who have been under the care of the Institution at their old establishment, during the year 1821. Of these, 26 have been cured, 1 relieved, 27 have been discharged at the request of their friends, and 7 as improper objects, 3 have eloped, 5 have died, and 52 have been transferred to the Bloomingdale Asylum. Of the foregoing, there were 13 whose insanity was caused by the intemperate use of spirituous liquors. Beside the 52 maniac patients transferred as before mentioned, there have been received at the Bloomingdale Asylum since the 27th July, 1821, 75 males and 48 females ; in all 123 : of whom 19 have been cured, 7 relieved, 9 have been discharged at the request of their friends, and 3 as improper objects, 2 have died, and 83 remained on the 31st December, 1821. Of the foregoing 123, there were 19 whose insanity was occasioned by the intemperate use of spirituous liquors.

The whole amount of expenditures for the support and maintenance of the Hospital for the year 1821, is $23,033 24; and the receipts during the same year, including the annuity of $12,500 allowed by the state, the sums received from the U. States, for sick and disabled seamen, from the several counties in the state, for the support of lunatic patients, up to the 27th July last, and all other sources of revenue, except the annuity granted for the erection of a building for lunatics, amount to $30,569 23; leaving a balance in favor of the Hospital of $7,535 99; which being deducted from $8,413 20, which the Hospital was indebted on the 31st December, 1820, leaves a balance against the Hospital, and for which it was indebted on the 31st December, 1821, of $877 21.

In 1816, the Legislature granted to the Governors an annuity for a term of years, to enable them to erect a building for the accommodation of lunatic patients, in which they might be enabled to employ the system of moral treatment which had been found efficacious in Europe, and which renders unnecessary much of that harsh usage to which such patients have generally been subjected. As this grant was made in trust for a particular object, the Governors have caused the accounts in relation to it to be separately kept. For the purchase and improvement of land, and for erecting and finishing the building, they have borrowed large sums of money at an interest of six per cent. and they have applied the annuity in part to pay the interest of that debt, and in part to form a sinking fund, which may finally discharge the principal. The cost of the whole establishment, including furniture, &c. has been $177,214 90; of which the sum of $165,000 is now owing by this corporation; and the amount of the sinking fund on the 31st December last was $21,257 26. The receipts and sums due for patients, since the new building was opened, have been $5,719 84 and the expenditures during the same period have been $7,637 23; but the latter includes the cost of many articles which will not soon require to be replaced; so that the ordinary annual expense cannot be accurately computed from that of the last six months. The building, which has been named the Bloomingdale Asylum, was opened in June last, but all the lunatic patients under the charge of the Governors, were not removed to it until in the month of July.

As the establishment is now complete, a brief account of its situation and management may not be unacceptable to the Legislature.

The building is of stone, three stories high, and about 211 feet long, finished in a style plain, but neat, and contains accommodations for about 200 patients. It is situated in one of the most healthful and beautiful spots on the island of New-York; commanding an extensive and variegated prospect. Annexed to it are about 70 acres of land, a portion of which is laid out in walks and gardens. The Trustees of Columbia College having presented to the Governors the plants that remained in the hot-house of the Botanical Garden, these have been placed in a proper building, and contribute to the embellishment of the place, and the amusement of the patients. With every local advantage, the Governors have commenced the system of moral treatment which the Legislature had in view when they provided for this institution; but without neglecting any of the aids which the science of medicine can afford. The medical department is conducted by a resident physician, who resides constantly in the house, and by an attending physician who visits it every week, and as much oftener as circumstances require. A superintendant and matron, respectable for their characters and deportment, have charge of the establishment, and the whole is superintended by a standing committee of the Governors, who visit it once a month, and of a sub-committee who visit it once a week. It is also inspected monthly by another committee, and monthly by the President and Vice President: and once in every three months there is a special visitation by the Governors, and such physicians, members of the city corporation, and other respectable citizens, as are invited to attend. By means of this perpetual supervision and inspection, abuses, if they should exist, cannot escape detection; and improvements are adopted whenever there appears to be room for them.

With a view to remove the melancholy train of ideas which, in many cases of insanity, take strong possession of the mind, salutary employment is provided for the patients; and when the weather and their health permit, they walk abroad in the garden and grounds, accompanied by the superintendant, matron, resident physician, or keepers. Tame animals have also been procured for them, as well as the means of other innocent amusements, such as ninepins, &c. They are permitted to dine together in classes, and their deportment at meals is much more orderly than might be expected. The utmost cleanliness is preserved, and nothing is neglected which it is supposed will contribute to their health and comfort. The bible is occasionally read to them, and on the first day of the week, those whose situation will permit, are assembled to hear a sermon, or some portion of scripture. Some friends of the institution have made it a donation of books, and thus laid the foundation of a library, for the benefit of such patients as are capable of using it. All those means which experience has shown to be efficacious in removing or alleviating mental diseases, are sedulously employed.

It would not perhaps be fair to decide too positively upon the merits of the mode of treatment that has been pursued, from the result of only six months' experience ; but thus far it has not disappointed the expectations that had been formed of it. It has proved eminently successful, and particularly so with regard to the patients who were at first most violent and noisy : a large proportion of these are greatly improved, and sanguine hopes are entertained that many of them will shortly be restored to their families and friends, blessed with mental as well as bodily health. Among those removed from the Old to the New Asylum, were several whom it had been necessary to subject to constant personal restraint : this has now ceased to be requisite in every instance, except that of a single individual, who is the only patient that at present requires it.

The Governors indulge a lively hope that this establishment, so honourable to the state, will be no less serviceable to humanity ; that it will abridge the amount of human misery, alleviate the sufferings occasioned by one of the most deplorable maladies to which our nature is subject, and acquire for the Legislature which has founded it, the gratitude of posterity.

MATTHEW CLARKSON, *President.*

THOMAS BUCKLEY, *Secretary.*

New-York, Febuary 5, 1822.

UNIVERSITY OF THE STATE OF NEW-YORK.

At a meeting of the Regents of the University, held at Albany, on the 11th of January last, the following Promotions and Appointments were made in the College of Physicians and Surgeons in the City of New-York :

WRIGHT POST, M. D. President, in the room of SAMUEL BARD, M. D. deceased.

DAVID HOSACK, M. D. Vice-President, in the room of WRIGHT POST, M. D.

JACOB DYCKMAN, M. D. and JOHN B. BECK, M. D. Trustees.

At a subsequent meeting of the Regents, the following appointment was made in the College of Physicians and Surgeons of the Western District:

JAMES M'NAUGHTON, M. D. Professor of Anatomy and Physiology, in the room of DELOS WHITE, M. D. resigned.

UNIVERSITY OF THE STATE OF NEW-YORK.

College of Physicians and Surgeons, City of New-York.

Professors, agreeably to the ordinance of the Honorable the Regents of the Univerity, February, 1822.

WRIGHT POST, M. D. President and Professor of Anatomy and Physiology.

DAVID HOSACK, M. D. F. R. S. Vice-President and Professor of the Institutes and Practice of Medicine.

WILLIAM J. MACNEVEN, M. D. Professor of Chemistry.

SAMUEL L. MITCHILL, M. D. Professor of Materia Medica and Botany.

WILLIAM HAMERSLEY, M. D. Professor of Clinical Practice.

VALENTINE MOTT, M. D. Professor of Surgery.

JOHN W. FRANCIS, M. D. Professor of Obstetrics and the Diseases of Women and Children.

Annual Commencement of the College of Physicians and Surgeons, City of New-York.

Agreeably to an ordinance of the Honourable the Regents of the University of New-York, the public commencement was held in the Hall of the College of Physicians and Surgeons, in the city of New-York, on the second day of April, 1822, when the following gentlemen were admitted to the degree of Doctor of Medicine, after having complied with the statutes of the University, and defended their respective Inaugural Dissertations.

NOVA-SCOTIA.

1. EBENEZER FITZ HARDING, *on the Management of the Placenta.*

CONNECTICUT.

2. RUFUS BLAKEMAN, *on Melancholia.*

3. STEPHEN MIDDLEBROOK, *on Puerperal Fever.*
4. HENRY PALMER, *on Dropsy.*

NEW-YORK.

5. SAMUEL BORROWE, JUN. *on Iritis.*
6. JOHN CADLE, A. M. *on Mortification.*
7. JOHN P. EMMET, *on the Chemistry of Animated Matter.*
8. PETER FORRESTER, A. B. *de Morbis Artus Genu.*
9. NATHAN S. JARVIS, *on Angina Pectoris.*
10. FERDINAND LUDLOW, *on Hydrophobia.*
11. ROBERT P. MACOMBER, *on Menstruation.*
12. JOTHAM W. POST, *on Diseases of the Hip Joint.*
13. DAVID L. ROGERS, *on Carotid Aneurism.*
14. WALTER S. SMITH, *on Emetics in Hysteria and Epilepsy.*
15. LEWIS TRAVER, *on Hepatitis.*
16. SPENCER WOOD, A. B. *on Hereditary Diseases.*
17. ANDREW VAN DYCK, *on the Topography and Diseases of Kinderhook.*

NEW-JERSEY.

18. JOHN S. CONDIT, A. B. *on the Lachrymal Gland.*
19. LEWIS D. FORD, *on Dysentery.*
20. THOMAS L. SMITH, *on Cataract.*

VIRGINIA.

21. HORACE AMES, *on the Phelgmasiæ.*

NORTH-CAROLINA.

22. BENNET BODDIE, *on the Functions of the Skin.*
23. SAMUEL GRIER, *on Hepatitis.*
24. WILLIAM H. HUNTER, *on Dyspepsia.*

SOUTH-CAROLINA.

25. WILLIAM E. ELLERBE, *on Urethritis.*
26. AARON LOPEZ, *on the Importance of Nosology.*
27. DAVID I. MEANS, *on the Momordica Elaterium.*
28. EDMUND C. PARK, *on Catarrhus Senilis.*
29. WILLIAM PORCHER, A. B. *on the indigenous Gentians of the United States.*

GEORGIA.

30. FITZGERALD BIRD, *on the Sanguinaria Canadensis.*

UNIVERSITY OF THE STATE OF NEW-YORK.

College of Physicians and Surgeons of the Western District of the State of New-York.—Established at Fairfield, (Herkimer County.)

From a circular which has just reached us, we are happy to learn that this institution is in a prosperous condition. Sixty-two students have attended the medical lectures during the past winter, being a larger number, we believe, than that of any preceding year. From its connection with the University of the state, the decided disposition to patronize it, which the Regents have lately evinced, and from the character and talents of the professors, we augur very favourably of the ultimate success of this establishment.

The officers of the College are,

Hon. JOSEPH WHITE, M. D. President, and Professor of Surgery.

WESTEL WILLOUGHBY, M. D. Professor of Obstetrics.

JAMES HADLEY, M. D. Professor of Chemistry and Materia Medica.

T. ROMEYN BECK, M. D. Professor of the Theory and Practice of Physic and of Medical Jurisprudence.

JAMES M'NAUGHTON, M. D. Professor of Anatomy and Physiology.

At the late Annual Commencement held in this College, the degree of Doctor of Medicine was conferred on the following gentlemen, who had respectively attended two complete courses of lectures, and exhibited and defended a thesis, viz.

MARTIN L. BRYAN, Fairfield, Herkimer Co.
ALEXANDER BURNSIDE, York, U. C.
SAMUEL FOOT, JUN. Chautauque Co.
HEZEKIAH GATES, Oneida Co.
LESTER GREEN, do.
REUBEN H. HART, Saratoga Co.
LESTER HOLT, JUN. Otsego Co.
EBER W. HUBBARD, Jefferson Co.
JAMES W. MILLER, Montgomery Co.
DANIEL D. PAGE, Oneida Co.
JARED H. Parker, Onondaga Co.
ELISHA POWELL, JUN. Saratoga Co.
SOLOMON P. SHERWOOD, Middlebury, Vt.

Case of ligature on the external Iliac Artery.—The operation of tying the external iliac artery was performed by Doctor Alexander H. Stevens, in the New-York hospital, on the 12th of March last, upon a young soldier who was admitted that day with a punctured wound of the femoral artery, just below Poupart's ligament. He had been put under guard on the 30th of April; and in attempting to pass the sentinel, received his bayonet on the outer and anterior part of the thigh. Profuse hemorrhage ensued, which was stopped by compression. It returned at intervals, until the time of his being brought to the hospital, when the limb was greatly swollen, and injected with blood—a firm coagulum plugged up the wound, and thin bloody serum oozed out of it. The skin around the wound was livid, and the man was in a state of high constitutional irritation. His pulse 136. The operator made an incision about two and a half inches long over the centre of Poupart's ligament, half an inch above, and parallel to it, except a slight curve upwards near its inner extremity. The layers of muscles were successively divided upon a director introduced under them. The peritoneum being then pushed up with the fingers, the sheath of the artery was laid bare, and cautiously divided. This part of the operation was rendered tedious by reason of the elevation of Poupart's ligament, and the effusion of blood into the sheath of the vessel. The artery being fairly exposed, the blunt needle of Drs. Parish, Hewson, and Hartshorn, was readily passed under it from within outwards, conveying a ligature of strong silk doubled. The operation lasted thirty-five minutes; and the man expressed himself as more comfortable soon afterwards. The limb, with little artificial aid, preserved its natural temperature—occasionally becoming hotter than the other. On the twelfth day a slight pulsation was perceived in the anterior tibial artery. The ligature separated on the twenty-second day; and after copious suppuration from the wound of the bayonet, the patient is now considered out of danger.

FOREIGN.

Vaccination in France.—In a critical analysis of a "*Report from the Central Committee of the Society for propagating Vaccination, on the number of children vaccinated in France, during the years* 1818, *and* 1819," by Dr. Granville,* some facts are stated of so interesting a nature, that we cannot deny ourselves the pleasure of extracting a few of them into this Journal.

The first of these facts relate to the irregularity observed in the progress of the pock after inoculation. Cases have been known to occur in which the pustule has furnished, after the fourth day, well-formed lymph, from which other children have been vaccinated, when the pustule was found to have lost that degree of precocity. Instances where the first appearance of the insertion having succeeded, manifested itself on the seventh, eighth, ninth, and as late as the thirteenth day, have occurred on various well-authenticated occasions.

It is now placed beyond doubt, that the vaccine matter will occasionally give rise to the spontaneous eruption of many more pustules in different parts of the body, the matter of which is perfectly good for inoculation. Of this fact we have ourselves been witnesses in more than one instance; and it goes far, in our opinion, to show that there is a general action produced by the vaccine virus on the constitution beside the local effect which arises from it.

Thirteen medical men have seen examples of vaccination proving the means of curing other eruptive diseases, the *crusta lactea* in particular; and it stands recorded in the present Report, upon unimpeachable authority, that the inoculated cow-pox has effectually cured scrofulous swellings, ophthalmia, chincough; and has appeared to exert a beneficial influence in cases of epilepsia, hemiplegia, and sciatica.

The Report is full of the experiments that have been repeatedly made, with a view to ascertain the efficacy of vaccination in subjects who were subsequently inoculated with the small-pox, or exposed to its contagion. The result has been uniformly favourable to the doctrine of the immortal Jenner. Four instances are detailed, of mothers attacked with small-pox, safely suckling their infants that had been vaccinated.

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* London Medical and Physical Journal for November, 1821.

All the above facts, many of which have been observed before, were confirmed during the prevalence of varioloid epidemics, which have of late years partially ravaged some of the provincial districts of France. Mr. Lejeune, for instance, relates that in the village of Claye, (department of l'Aisne,) of thirteen children, one only, that had been vaccinated, resisted the small-pox, by which the other twelve were attacked, and of which four of the twelve died.

The influence which the cow-pox seems to exert over the small-pox, has been strongly illustrated in those individuals who, having been vaccinated after exposure to the variolous contagion, have experienced a disease extremely mild in all its characters.

The Report lays great stress on the comparative rarity of small-pox in places where vaccination has been much and generally practised."

The Report is signed by Chaussier, president; Corvisart, Delasteyrie, Doussin-Dubreuil, Hallé, Huzard, Jadelot, J. J. Leroux, Parfait, Pinel, Salmade, Duchanoy, Alibert, Bourdois, Guerbois; and Husson, secretary; and is followed by an ordinance of the minister of the interior, decreeing the distribution of one premium of 3,000, two of 2,000, and three of 1,000 francs each, to the individuals who most distinguished themselves in propagating vaccination; and the awarding of gold and silver medals, for the same reason, to upwards of two hundred persons, all of the medical profession, with one single exception.

" A voluminous Table follows the Report, containing the names of the departments, their population, the number of births, of those vaccinated, those attacked with the small-pox, those rendered infirm or disfigured by that disorder, those who died of it; the proportion of vaccinations to the number of births, the names of the operators; the amount of the expenditure for promoting vaccination; and general observations. The total result taken from this Table, occupying twenty-six pages, may be collected from the following recapitulation :

	Population	Births.	Vaccinations.	Subjects who hed the small pox.	Deformed or infirm from it.	Died of small pox.	Proportions of Vaccinations to Births.
81 Departs. 1818	. . . . .	722,589	442.945	43,939	2,392	6,171	1 in $1\frac{1}{3}$
81 Departs. 1819	. . . . .	835,550	425,510	52,036	4,264	6,586	1 in 2
Total. . . .	27,135,119	1,558,139	868,455	95,975	6,656	12,857	1 in $1\frac{1}{2}$

Amount of expenses in the two years, ... 1818. . . . . 95,292,57; 1819. . . . 101,914,33 } 198,206,90 fr.

On the use of Carbonate of Iron in Neuralgia, or Tic Douloureux.—The English Journals continue to confirm the beneficial effects resulting from the use of carbonate of iron, in cases of Tic Douloureux, a practice suggested not long since by Dr. William Hutchinson of London. The Medical Repository of London, for May, 1821, contains the detail of a case of this disease, by Dr. Carter, in which blisters, lotions of various kinds, opium, bark, and ammoniated tincture of Guaiacum had been used successively, for a length of time, without in the least mitigating the tortures of the patient. At last recourse was had to the carbonate of iron. On the 13th of January, he began taking a scruple every three hours, made into an electuary, with confectio. aromat. and syrup. 26th, had been quite free from pain for four days. His appearance was much improved, and he complained of nothing but numbness of the side of the face. February 3d, no recurrence of pain. Iron discontinued. February 8th, numbness nearly gone. No return of pain. He remained perfectly well.

To the same effect are several cases related by Drs. Richmond and Hutchinson, in the London Medical and Physical Journal, for September and November, 1821. Dr. Hutchinson does not by any means contend that iron is a *specific* in this disease. In his observations on this subject, he says, " a professional experience of nearly thirty years has taught me not to place imiplcit confidence in the curative and specific agency of any remedy at present known. We are not in these days to be taught, that the bark will not always subdue the paroxysms of intermittent fever, and that even mercury* has been known to fall short of the subjugation of syphilis. The carbonate of iron has, in a few instances under my own cognizance, been inadequate to the difficult task of curing the tic douloureux. I can however assert, that, in these very few instances, it has done more towards alleviation than any other known remedy. The carbonate of iron has been frequently given in very improper cases, and in the most injudicious manner, without any due consideration of peculiarities of constitution, or of the proximate cause of the disease; circumstances essentially requisite to be attended to in the administration of a powerfully stimulant remedy. In the very few unsuccessful cases under my own management, I can almost uniformly attribute the want of success rather to the absence of a due perseverance in the use of the remedy, than to any failure in its power. It ought also to be perfectly understood that collateral means of relief have uniformly been used in conjunction with the iron, and that the treatment is by no means confined to one remedy. Where symptoms of active inflammation have existed, the means usually employed to subdue inflammation have been had recourse to, and the iron either suspended or never used."

As a specimen of the general mode of treatment pursued by Dr. H. we select the following case:

"At the expiration of a year's suffering, Mr. Todd applied to me; and I immediately commenced that plan of management which experience had taught me to be the most successful. In the middle of last May he began to take one drachm of the carbonate of iron three times a-day; and I directed him to make use of an ointment to the affected cheek every night, consisting of the tartrite of antimony, powdered opium, camphor, and mercurial ointment;

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* "The fact that it [mercury] acts with equal rapidity and effect upon all cases of inflamed iris, whatever their origin, seems to me to show that the idea of a *specific* antisyphilitic virtue possessed by this mineral is an erroneous one."—*Vide* COOPER'S *and* TRAVERS' *Surgical Essays*, Part I. 3d edition.

one drachm of which was used each evening, until a plentiful crop of pustules appeared : its use was then suspended a few days, and renewed when the eruptions were healed.

This plan was duly persevered in for three weeks, when my patient had experienced some very trifling degree of relief. The medicine now produced uneasy sensations in his bowels, which were removed, and in future prevented by mixing the powder with the confect. aromatic. instead of honey ; and, if diarrhœa at any time accompanied its exhibition, five drops of the tinct.opii added to each dose restrained any inordinate action of this nature.

During the ensuing six weeks my patient pretty regularly persevered in the same plan, modifying and altering the treatment according to existing symptoms, occasionally omitting the medicine a day or two from unforeseen circumstances. During this period he gradually lost his pains, and he is now perfectly well.

Hydrophobia.—An interesting memoir on Hydrophobia has lately been presented to the public, by M. Marochetti, a Russian Physician, which throws new light on this disease, and opens a new field for medical inquiry. The memoir was first read before the Medico-Physical Society of Moscow, and afterwards printed at St. Petersburgh, from whence a copy of it was transmitted to our friend Dr. Mitchill, by Mr. Middleton, Minister Plenipotentiary from the United States to the Russian Court. Dr. Marochetti affirms, that before the discovery which he announces, physicians were ignorant of the true seat of this disease. He professes to point out certain indications for discovering the existence of the hydrophobic poison, and the means of destroying its activity in an infected subject. The following are the conclusions which he states he has drawn from a most scrupulous attention to hydrophobia.

In the first place, he is convinced by experience, that if several persons are bitten in succession by a rabid animal, the first bitten exhibits in the development of the disease, more serious and violent symptoms than the second, this more than the third, and so on ; the poison acting always in an inverse ratio to the numbers, in such a manner that the eighteenth or twentieth person may be considered out of danger. Such a case sometimes happens.

Secondly, the hydrophobic virus does not constantly reside i the mouth of the mad animal.—It gathers there only at the end of a certain time. The bite, in this interval, is not to be considered as venomous. Here is another case in which the rabies is not communicated.

Thirdly, the hydrophobic virus does not lose, like the pestilential miasma, any of its intensity in being communicated from one body to another—but it acts with more or less violence in proportion to its quantity. Unfortunately, the effect is not the less fatal, by reason of its quicker or slower operation.

Fourthly, it is evident that this virus does not continue in the wounds; but is conveyed in its full force to a part of the body presently to be described. In this part, it instantly acts as a most powerful *astringent*, and, by accumulation, inflames and shuts the passages, by which nature endeavours to expel it from the animal economy.

Fifthly, there is one, and only one way, to prevent the development of hydrophobia in a person who is threatened with it. He declares boldly, and experience he says will bear him out in the assertion, that this is to evacuate the hydrophobic virus when it appears. Where does this reside ? and how can it be evacuated ?

Sixthly, the sub-lingual glands are two in number, one on each side, under the tongue, between the genio-glossi muscles, the lower jaw, and the inner membrane of the mouth, which immediately covers them. From these glands proceed two or three secretory ducts, which open into the ducts of the sub-maxillary glands, and those latter open one on the one side, and the other on the other side of the frenum of the tongue—it is precisely to the extremities of these ducts that the hydrophobic virus is conveyed after a bite inflicted by a rabid animal, and there it is *temporarily* detained, forming at the two spots just described, one or two small tumours of unequal size. By touching, which may be done with a probe, it is found that there is contained a fluctuating humour, which is, as absorption proves, the hydrophobic poison itself. It is there that nature delivers to us her enemy. It is from this hold that the surgeon ought to expel him.

Seventhly, the time cannot be exactly defined at which these small tumours will show themselves. If the virus is not evacuated in 24 hours it disappears by *re-absorption*. Then no trace is left of its pre-existence. There takes place a double metastasis toward the brain. The most frightful symptoms of hydrophobia commence, and the patient sinks under one of its paroxysms.—On opening the body, nothing remarkable is observed; pathological anatomy, after all its explorations, has not discovered any indication capable of fixing the attention of physicians, and of rendering an explanation of the causes, because they did not understand the *re-absorption* of this virus.

Eighthly, the first thing to be observed when a person believes he has been bitten by a mad animal, is the lower side of the tongue, which ought to be examined for six weeks, once, or even twice a day, for more perfect precaution. If at the end of this term, there is no appearance of the small swellings already described, we may rest assured that the individual is not to be infected with hydrophobic virus. During these examinations, if the tumours appear they must be instantly cauterized, or, what is better, opened with a small sharp lancet. This operation is performed by raising the tongue with one hand, covered with a suitable cloth, toward the palate, and somewhat sideways, to the end that the small buttons or tumours may be easily touched. For a greater convenience the tongue ought to be supported by an assistant; so that there may be made as many longitudinal incisions as there are lumps. From these latter there will proceed some drops of a sanious lymph, somewhat greenish, which the patient operated upon, will spit out. As soon as the operation is over, the patient must wash his mouth with a strong decoction of the tops and flowers of the Genista lateo-tinctoria. It is almost superfluous to observe, that this decoction must be prepared in advance, for it must be regularly taken, during the six weeks that the patient is under examination, as a common drink. The dose is a pound and a half per day, in decoction; or four drachms a day in powder, in doses of one drachm each. The prescriber, however, will always take into consideration the age and constitution of the patient.

Ninthly, he observes that the operation, which consists in opening the small tumours, has the great advantage of being so simple and so easy to execute, that not only professional men, but every individual who has seen it performed two or three times, can perform it himself. This is very important for the small towns and villages, where, as yet, there are no settled and established surgeons,

For his discovery of the seat of this disease, and the method of treating it, Dr. Marochetti acknowledges himself indebted to a Russian Peasant living near the village of Kigawka, who, it is said, has for a number of years made a business of attending persons bitten by hydrophobic animals, and with uniform success. This plain man begins by examining, every morning and evening, the lower or under part of the tongue of the individual bitten; and as the sublingual tumours appear, he opens and cauterizes them with a sort of large needle heated red hot in the candle, and causes the patient to rinse the mouth with the decoction of the genista, which is also used internally. Of fourteen persons bitten by one hydrophobic dog, and treated by the Peasant under the immediate observation of Dr. M., twelve underwent the operation of opening their sublingual tumours, and were saved. The two others who were bitten last, had no tumours. These fourteen persons, after having for six weeks made use of the decotion of genista, were discharged cured.

Dr. Marochetti states that of twenty-six persons who were bitten by one hydrophobic dog, and successfully treated by him, nineteen had swellings under the tongue. Of these individuals, they who had the deepest and most numerous wounds, experienced the tumours on the third day; the rest on the fifth, seventh, and ninth days; and in one woman the swellings did not appear until the twenty-first day after the bite.

The succession of the precursory symptoms, observable during the formation of the small pustules or tumours, requires the greatest attention. Towards that stage, says Dr. M. the pupil of the eye is dilated and fixed; the look is sad; uneasiness is complained of; and there is a small pain in the head. These are the only symptoms he has been able to detect.

After the disclosures he has made concerning the hydrophobic virus, Dr. M. considers the disease as a local evil, of which it is possible to prevent the consequences by a seasonable removal of the morbific matter. For after the re-absorption has taken place, there are no means of arresting its terrific effects. He thinks it may be thence concluded, that the known symptoms of hydrophobia, such as constriction of the breast, violent pain in the head, dilation of the pupil, occasional palsy of the tongue, convulsions, dread of liquids, all nervous symptoms, are produced by the re-absorption of the hydrophobic poison.

Injections of Spirit of Turpentine in cases of obstinate Constipation. In the London Medical Repository for September, 1821, a case of obstinate Constipation is recorded, in which, after all other remedies had failed, an injection of Spirits of Turpentine proved successful in opening the bowels. The patient had passed about thirteen days without any alvine evacuation. His symptoms at last became so urgent, that Mr. Williams, the surgeon in attendance, determined to have recourse to the use of Turpentine. He accordingly beat up an ounce of spirit of Turpentine, and the yolk of two eggs, with half a pint of gruel, and succeeded in throwing up about four ounces. It occasioned an immediate tenesmus and smarting at the anus, and in less than half an hour his patient had a full and copious motion; the hiccup and vomiting, which before this had been very distressing, ceased, and he got several hours sleep. Before night he had another evacuation, and the next day five more natural stools, after which he continued slowly to recover.

Cathartine, the active Principle of Senna.—MM. J. L. Lassaigne and H. Fenuelle, in examining senna, obtained from it a particular principle called by them cathartine. A decoction of the leaves was made, and, after being filtered, was precipitated by acetate of lead. The precipitate collected was diffused through water, and sulphuretted hydrogen passed through it. The liquor filtered was evaporated to dryness, and digested in alcohol, and the alcoholic solution then evaporated to dryness. It contained acetate of potassa, which was separated by alcohol acidulated by sulphuric acid: then filtering to separate the sulphate of potassa insoluble in this fluid, precipitating the excess of sulphuric acid by acetate of lead, decomposing this latter salt by sulphuretted hydrogen, filtering again and evaporating to dryness, a substance was obtained, which was considered the purgative principle of senna.

This substance is uncrystallizable, of a reddish yellow colour, of a particular smell, a bitter and nauseous taste. It is soluble in alcohol and water in all proportions; insoluble in ether. Its extract becomes moist in the air. It purges in very small doses.—*London Med. Repos. for September*, 1821.

Piperin, or the active Principle of Pepper.—Piperin is a new vegetable principle, extracted from black pepper, by M. Pelletier. To obtain it, black pepper was digested in alcohol repeatedly, and the solution evaporated, until a fatty resinous matter was left. This, on being washed in warm water, was left of a good green colour, and had a hot and burning taste; it dissolved readily in alcohol, and less readily in sulphuric ether; concentrated sulphuric acid gave it a fine scarlet colour. A solution of this substance in hot alcohol, being left for some days, deposited a number of small crystals. These were purified by repeated solution and chrystallization in alcohol and ether, and from the mother liquors, fresh portions were obtained, which, on purification, were like the first. It is to be remarked, that the pepper taste they possessed when impure, gradually left them as they became more and more pure; so that the white crystals scarcely had any taste, while it seemed to accumulate in the fatty matter, as the crystalline portion was separated from it: and also, that the purer the crystals, the finer the tint produced in them by sulphuric acid. The fatty matter left, also reddened by sulphuric acid; but it is a question whether it would do so when pure.

The crystalline matter forms colourless four-sided prisms, with single inclined terminations; they have scarcely any taste. Boiling water dissolves a small portion; but it is insoluble in cold water; it is very soluble in alcohol, less so in ether; it is soluble in acetic acid, and crystallizes from it in feathery crystals. Weak sulphuric, nitric, and muriatic acids, do not dissolve or act on it; the strong acids decompose it. Strong sulphuric acid gives it a blood-red colour, which disappears on adding water; the substance does not seem altered if the acid has not remained long on it. Muriatic acid acts in the same way, producing a deep yellow colour. Nitric acid makes it greenish yellow, orange, and then red; the ultimate action of the acid produces oxalic acid, and yellow bitter principle. It melts at about 212°. Destructive distillation converts it into water, acetic acid, oil, and carburetted hydrogen gas: no ammonia is formed. Oxide of copper converts it into carbonic acid and water. After comparing this substance with other vegetable principles, particularly with resins, M. Pelletier concludes by considering it a peculiar substance, and names it *Piperin.*

The fatty matter left, after extracting the piperin, is solid, at a temperature near 32° ; but liquefies at a slight heat. It has an extremely bitter and acrid taste ; it is very slightly volatile, and tends rather to decompose, than rise in vapour ; that which passes over is not so piquant and acrid as the undistilled part, but is more balsamic. It dissolves easily in alcohol and ether, and unites to fatty bodies ; and, with the exception of its taste, does not differ from them. From the result of the distillation, it may be considered as composed of two oils : one volatile and balsamic ; the other more fixed, and containing the acridness of the pepper.

Finally, M. Pelletier finds in pepper—1, Piperin ; 2, a very acrid concrete oil ; 3, a volatile balsamic oil ; 4, a gummy coloured matter ; 5, an extractive principle ; 6, malic and tartaric acids ; 7, starch ; 8, bassorine ; 9, lignin ; 10, earthy and alkaline salts. He concludes, also, there is no vegetable alkali in pepper ; that the crystalline substance of pepper is a peculiar body ; that pepper owes its taste to an oil but little volatile ; and that a strong similarity exists between common pepper and cubebs, as illustrated by the analysis of M. Vauquelin, of the latter compared with the former.—*Ib.* p. 337.

Nitrate of Potass.—Mr. Tordeux, an apothecary at Cambrai, has had occasion to examine some hexaedral and circular crystals, which he found in a mass of the extract of cochlearia officinalis that had been lying by for a considerable length of time. They consisted of nitrate of potash, which salt Mr. Tordeux has since been able to obtain from the same plant, by means of subacetate of lead, sulphuric acid, and concentrated alcohol. The presence of nitrate of potash in the cochlearia would explain the diuretic properties which that plant is known to possess.—*London Med. and Phys. Journal, for Nov.* 1821.

Sarcocele.—M. Maunoir, of Geneva, has recently recommended to the surgical profession a mode of treating sarcocele, without extirpation of the testicle. This consists in cutting off the main current of blood from the organ, by tying the spermatic arteries.

He recommends these vessels to be tied close up to the ring, so as to interrupt the circulation through as many of the small branches as possible. Previous to an operation, however, it is necessary to distinguish sarcocele from medullary fungus of the testicle, with which, he thinks, it is too often confounded.

M. Maunoir put this proposal to the test in two cases. In one, the spermatic artery was tied on the 8th of June, 1820, and divided below the ligature. By the 4th of July, an evident diminution had taken place in the tumour. On the 20th of August the patient quitted the hospital, his testicle being without pain, and not larger than the sound one. Hydrocele was combined with enlargement of the testicle in this case, and the water had been twelve times evacuated, but still returned. The pain ceased the moment the artery was divided, and the hydrocele was radically cured. The suggestion deserves some attention from our surgical brethren.

Johnson's London Medico-Chirurgical Review.

Mr. J. Taylor, mining engineer, has made some remarks on Mr. Schoolcraft's account of the copper of the southern shores of Lake Superior. He denies that the discovery of masses of native copper is generally considered indicative of the existence of mines in the neighbourhood. It is an opinion which does not rest on the testimony of practical men. Although native copper frequently occurs in some of the best mines of that metal, it is comparatively a rare production, and does not form any notable proportion of their produce. In judging of the probability of any country being productive of copper, a miner should attend to the appearance or discovery of regular veins, to their extent, to the rocks in which they occur, the substances which they contain, and to many other things which he would deem more important than any casual specimens of the metal or its ores, and particularly the richer varieties.

With regard to the probability of copper being found in the country described by Mr. Schoolcraft, near Lake Superior, so as to become an object of research, it appears to Mr. Taylor that there is very little to encourage such an expectation. Scattered fragments of native copper are found inclosed in masses of rock not even *in situ*, their situation uncertain, the rock itelf not commonly productive of copper, the surrounding country alluvial, or composed of red sand-stone, no indication of veins, and no appearance of such substances as are most frequently found accompanying the ores.—*Thompson's Annals of Philosophy, January,* 1822.

M. Berzelius has lately examined the Alkaline Sulphurets. Vauquelin had before conjectured, that Hepar was a mixture of sulphate of potash and sulphuret of potassium, and his opinion was supported by the experiments of Gay Lussac. Berzelius has placed the question beyond doubt. Part of the potash is reduced to form the sulphuric acid, which unites with a part of what is not decomposed. The ordinary Hepar is considered as a mixture of one atom of sulphate of potash, with three atoms of sulphuret of potassium. Berzelius also found that potassium and sulphur unite in seven different degrees.—*Edinburgh Philosophical Journal,* 1822.

Corvisart.—The following account of the funeral of the celebrated CORVISART, who lately died in Paris, is extracted from a French Journal. We hope to be able to furnish, in our next number, a biographical sketch of this ornament of the Medical profession.

" The obsequies of M. le Baron de Corvisart, physician, took place to-day. In pursuance of the directions contained in his will, his body was not taken to the church of St. Elizabeth, his parish, but was conveyed to his estate at Atys, where he desired that the religious ceremonies should be performed. A deputation from the Faculty of Medicine, in their doctor's robes, (*en habit doctoral,*) and nearly all the physicians of Paris, repaired this morning to his late residence, Rue de Vendome, for the purpose of attending his funeral. Mr. LEROUX, *doyen* of the physicians of Paris, pronounced a discourse over his coffin. The body, covered with the mantle of a doctor, and with the different orders which had been conferred upon the deceased, was afterwards placed in a hearse, to be taken to its destination, whither the deputation of the Faculty, and a great number of other persons, accompanied it."

METEOROLOGICAL OBSERVATIONS

FOR JANUARY, 1822.

Made at the New-York Hospital, by Richard Pennell, M. D.

—	Barometer.			Thermometer.*			Winds.			Rain.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 A M	2 P M	9 P M	Gauge.	
1	30 20	30 00	30 00	26	40	38	sw	sw	sw		cloudy
2	30 00	30 00	30 00	32	38	30	sw	nw	nw		clear
3	30 00	30 00	30 00	22	38	36	nw	sw	sw		do
4	30 00	30 00	30 00	12	20	8	n	n	n		cloudy
5	30 00	30 00	30 00	0	19	16	nw	nw	nw		clear
6	29 90	29 90	30 00	18	30	29	sw	sw	sw		variable
7	30 28	30 28	30 20	15	40	30	nw	nw	nw	—.05	do
8	30 10	30 10	30 10	28	40	36	ne	ne	ne	—.95	cloudy
9	30 10	30 20	30 20	25	40	20	nw	nw	nw		clear
10	30 50	30 40	30 40	12	26	16	nw	nw	nw		cloudy
11	30 40	30 30	30 20	5	32	22	nw	nw	nw		clear
12	29 90	29 90	29 90	20	40	30	sw	sw	sw		cloudy
13	29 90	30 00	30 00	25	32	10	sw	sw	nw		do
14	30 33	30 33	30 30	–2	30	27	nw	nw	sw		variable
15	30 10	30 20	30 30	14	30	28	sw	sw	sw		do
16	30 60	30 70	30 90	25	40	19	n	n	n		clear
17	31 00	31 00	30 90	8	30	20	n	n	n		do
18	30 70	30 70	30 60	10	40	38	n	sw	sw	—.19	variable
19	30 40	30 40	30 40	40	45	38	sw	sw	sw		cloudy
20	30 40	30 40	30 40	30	45	36	sw	sw	sw		do
21	30 20	30 20	30 00	28	38	36	w	w	w		do
22	30 00	29 90	29 80	35	46	10	w	w	w		do
23	29 80	29 80	30 00	20	22	10	nw	nw	nw		do
24	30 10	30 10	30 30	0	10	13	nw	nw	nw		clear
25	30 40	30 40	30 40	0	22	8	nw	nw	nw		do
26	30 40	30 40	30 40	6	31	30	nw	sw	sw		cloudy
27	30 40	30 40	30 40	10	30	19	sw	sw	sw		do
28	30 30	30 40	30 40	10	40	20	sw	sw	sw		do
29	30 30	30 20	30 20	18	53	36	se	se	se		do
30	30 20	30 20	30 00	32	32	35	se	se	se	—.04	do
31	29 90	29 90	29 90	30	31	32	ne	ne	ne	—.12	do

* *The thermometer was suspended on the east side of the Hospital, between its two projecting wings; hence, the temperature at 2 P. M. was increased by radiant heat from the wall. In future it will be placed in a situation where the reflected rays of the sun can have little or no influence upon it.*

The quantity of rain which fell in January, is 1 inch and 35-100ths. The coldest day this month was the 14th, when the Mercury stood at 7 A. M. 2 degrees below Zero of Fahrenheit. Snow fell on the 6th and 8th in small quantities, which soon disappeared; on the 27th a sufficient quantity fell to afford good sleighing. There fell a little sleet on the afternoon of the 10th. The sun was obscured on the 4th, 13th, and 20th.

METEOROLOGICAL OBSERVATIONS

FOR FEBRUARY, 1822.

Made at the New-York Hospital, by Richard Pennell, M. D.

	Barometer.			Thermometer.			Winds.			Rain.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 A M	2 P M	9 P M	Gauge.	
1	29 90	29 90	30 00	32	37	25	ne	nw	nw		cloudy
2	30 40	30 40	30 40	28	50	29	nw	nw	nw		clear
3	30 40	30 40	30 40	20	48	37	sw	sw	sw		cloudy
4	30 00	30 00	30 20	32	40	30	sw	nw	nw		do
5	30 50	30 50	30 40	15	30	27	nw	nw	sw		clear
6	30 30	30 20	30 30	32	38	30	sw	nw	nw		cloudy
7	30 40	30 30	30 30	15	30	17	nw	nw	nw		do
8	30 30	30 30	30 20	12	38	25	w	w	w		do
9	30 00	30 00	30 00	15	40	30	nw	nw	nw		do
10	30 00	30 00	30 30	30	38	25	nw	w	w		do
11	30 00	30 00	30 00	20	34	27	sw	sw	sw		clear
12	29 90	29 90	29 90	20	36	34	sw	sw	sw		cloudy
13	29 90	29 90	29 90	25	32	23	ne	ne	ne		do
14	29 90	29 90	30 00	19	30	25	ne	ne	ne		do
15	30 35	30 40	30 40	10	40	26	nw	nw	nw		clear
16	30 40	30 30	30 20	25	40	38	sw	sw	sw	1.20	cloudy
17	29 90	29 90	30 00	38	40	40	sw	sw	sw	—.02	do
18	30 00	30 00	30 00	30	30	30	ne	ne	ne		do
19	30 04	30 04	30 04	15	40	20	nw	nw	nw		clear
20	30 30	30 30	30 30	10	40	22	sw	sw	sw		cloudy
21	30 00	30 30	29 90	48	55	48	se	se	se	1.23	do
22	29 90	30 00	30 00	45	60	46	se	nw	nw		variable
23	30 40	30 40	30 40	35	56	40	nw	nw	nw		clear
24	30 40	30 40	30 40	45	60	38	sw	sw	sw		do
25	30 30	30 30	30 40	32	32	30	nw	nw	nw		do
26	30 40	30 30	30 30	20	50	37	nw	sw	sw		variable
27	30 30	30 30	30 30	40	60	47	sw	sw	sw		clear
28	30 30	30 30	30 30	46	62	55	sw	sw	sw		variable

The quantity of rain which fell in February, is 2 inches 45-100ths.

It has been generally remarked, that this month was mild, and very unlike the same month in the preceding year. On the morning of the 4th, a small quantity of snow fell, wind S. W., changed at 1 P. M. to N. W., when the snowing ceased; at 9 P. M. lunar halo. On the 6th, at 4 P. M., fall of snow for the space of half an hour; wind N. W. On the 14th, snow at 8 A. M.; in the course of the day it fell to the depth of an inch and a half on the level. Snow, with a very strong wind from the S. E. on the 18th. On the 21st a gale from the S. E. brought a copious rain which melted the snow with astonishing rapidity. The flood thus produced was very destructive to bridges, mill-dams, roads, &c.; the ravages extending through the northern and middle states, particularly in the vicinity of the coast. The rain continued during the forenoon, but about 2 P. M. the clouds began to disperse, and the sun became visible a short time previous to the commencement of an eclipse. This interesting phenomenon was accurately observed with a good three-and-a-half feet achromatic telescope, by a gentleman of this city; but he regrets the going of his watch had not been previously adjusted.

The time noted is as follows: Commencement, 26 minutes 30 seconds past 3 P. M. End, 9 minutes 25 seconds past 5. Much lightning in the north at 9 P. M. The sun was obscured on the 6th and 13th.

Obituary.—Died recently at Albany, James Low, M. D. aged 39. Dr. Low was for a considerable period of his life eminently distinguished as a learned and excellent Physician. He graduated in the University of Edinburgh, in the year 1809, after having prosecuted his Medical studies under the particular direction of the late distinguished teacher of Chemistry, John Murray, M. D.

NOTICES.

The following work, just published by James V. Seaman of this city, has been received, and will be reviewed in our next number, viz. System of Surgical Anatomy. Part first, on the structure of the Groin, Pelvis, and Perineum, as connected with Inguinal and Femoral Hernia; Tying the Iliac Arteries; and the operation of Lithotomy, illustrated with nine copperplate engravings. By William Anderson, Licentiate of the Royal College of Surgeons in Edinburgh, and Lecturer on Surgical Anatomy in New-York.

An Essay on the Chemistry of Animated Matter, by John Patten Emmet, has also just been published. It shall be noticed in our next number.

Several interesting articles, on subjects connected with Medicine and Natural History, have been unavoidably excluded from the present number of our Journal.

NEW-YORK

MEDICAL AND PHYSICAL

JOURNAL.

VOL. I.] APRIL, MAY, AND JUNE, 1822. [No. II.

ART. I. EXPERIMENTS *for ascertaining the Permanency of Chemical Compounds in their Passage through the Fluids.* By WILLIAM JAMES MACNEVEN, M. D. Professor of Chemistry in the University of New-York. &c.

WHETHER the process of assimilation be so invariable and powerful as to convert all substances received by the stomach into bland nutriment, or whether some matters resist its sway and retain their own nature while they pass into the circulation, is a question still debated in this country.

The affirmative would, in all probability, have long since been abandoned, but that it has found an able advocate in the Professor of the Practice of Physic of the University of Pennsylvania, whose distinguished talents and high standing give just importance to the opinions he maintains.

Already, however, a graduate of the College of Physicians and Surgeons of the University of New-York, who reflects honour upon it by his genius and learning, has replied to Doctor Chapman with a force of logic and extent of information, that seemed to leave nothing to be desired or added.*

More recently, in the preceding number of this Journal, the review of Dr. Chapman's Therapeutics has again brought up the question. Again it has produced an able refutation, at the same time advantageously distinguished by the *suaviter in modo* and the *fortiter in re*, and exhibiting an example, which we hope will be followed, of conclusive argument and polite controversy.

We find the same subject employing the talents of Dr. Eberle in the 5th volume of the Medical Recorder. As if no part of it were now to be left untouched or undecided, the experiments, the arguments, the opinions of chemists, physiologists, and practitioners throughout Europe are, in this learned and able production, brought to bear upon the whole case in a manner, to us, perfectly decisive.

Indeed, the facts already known have been so well applied by various persons, if that any farther proof or elucidation be wanting, it can be only found in a renewed experimental examination of the fluids, with reference to the fact of heterogeneous substances being discoverable in them, possessed of the same properties with which they entered the chylopoietic organs. We are told† that satisfactory experiments of this kind have been performed by Dr. Harlan of Philadelphia, which confirm others made by Professors Tiedman and Gmelin; but as yet I have seen no detailed account of any of them. Moreover, it is always useful to repeat and vary experimental inquiries, until such time as the points in issue shall be settled by general agreement.

The following experiments were accordingly instituted, with the view of tracing from the stomach to the bladder a substance that may be safely taken in large doses, and one that is easy to distinguish. I was assisted in them by my colleague, Professor Francis, and by Dr. James Anderson, who opened the animals.

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* See Discourse of Thomas Cooper, M. D., now President of Columbia College, S. C., delivered before his Chemical Class, in the University of Pennsylvania, November, 1818.

† Medical Recorder, vol 5. p. 13.

1. I made a saturated solution of hydrocyanate of potassa, of which two ounces were mixed with a pint of milk, and given to a dog to lap. He took about one third part of this quantity, lay down, as if indisposed, and in about an hour vomited some. I tested what he threw up with a solution of permuriate of iron, and immediately obtained a blue colour. After this, the dog would not touch again such a mixture, even after two days of fast; so much was his sagacity alarmed.

2. I now triturated one drachm of chrystallized hydrocyanate of potassa with fresh butter and crumb of bread, which being made into a bolus, the same dog swallowed and retained.

Between three and four hours after, Dr. Anderson bled him largely from the jugular vein. A dose of hydrocyanic acid was then administered, of which he died without pain, and the abdomen was laid open. The lacteals and thoracic duct were seen well filled with milk-white chyle. On scratching the receptaculum, and pressing down the duct, nearly half a tea spoonful of chyle was collected. Into this were let fall a couple of drops of the solution of permuriate of iron, and a deep blue was the immediate consequence.

The urinary bladder being taken out, and the urine tested after the same manner, the result was also similar.

Before the death of the animal some fæces escaped involuntarily, and, as might be expected, they gave a Prussian blue with the permuriate of iron.

The next day, there were mixed with a portion of the serosity, for it was tinged red, a few drops of the solution of iron. They made it coagulate and turn to a greenish yellow. A drop of dilute hydrochloric acid was added, and it had the effect of changing the whole to a shade of light blue.

It results from these experiments, that the hydrocyanate of potassa was not decomposed in the stomach, nor in the intestines, nor in the lacteals, nor in the bladder; and that faint traces of it were discernible in the serum of the general circulation.

We repeated the experiment on another dog, with a view chiefly to try the effect of the iron test on serum from the venaportæ. The animal was made to take a drachm and a half of hydrocyanate of potassa at short intervals, and bore it without inconvenience. He was killed with hydrocyanic acid, and then opened. As soon as the serum had separated, a portion of it was introduced into a small tube of white glass, and a few drops of the solution of permuriate of iron added. By rolling the mixture through the tube, the sides of it, where they transmitted light, exhibited a very distinct shade of blue. Persons, who knew nothing of the experiment, found it so, as well as half a dozen students who witnessed this second experiment.

Wherever the mesenteric vessels, or the external coats of the intestines, were lightly scratched with the scalpel, and touched with the solution, a strong blue was immediately exhibited.

The solution produced no change in the cystic bile, nor on the substance of that viscus.

Dr. Ducachet obtained a result of the same nature, of which he gives me the following account:

"June 5th, 1817. In the presence of Professor Francis, I performed the following experiment. I gave a cat about half a pint of milk, deep tinged with indigo. In half an hour I killed the animal; and upon dissection found the lacteals containing a fluid of a deep blue colour. In the lower part of the intestinal tract, the lacteals were perfectly white, the milk not having passed so low down. This experiment was made for the purpose of testing the truth of Musgrave's assertions respecting the absorption of foreign substances in the chyle."

In order to conclude with the greater caution from these experiments, I undertook to elucidate them by others made on serum actually containing hydrocyanate of potassa, which I dissolved in it myself. And having read in a paper of Sir Everard Home, in the Philosophical Transactions for the year 1808, p. 53. that less than a grain of prussiate of potash to two ounces of serum would not produce a blue colour on the addition of a solution of iron, I dissolved one grain of hydrocyanate of potassa in two ounces of clear human serum held in a graduated measure.

About a drachm of this mixture was put into a test tube of white glass, and a couple of drops of the same solution of permuriate of iron were added. The serum was coagulated, and a deep Prussian blue produced. In the experiments that followed, I poured off each time an ounce of the mixture, and added an ounce of fresh serum, so as to leave the quantity of hydrocyanate of potassa in every succeeding experiment, just half what it was in that immediately preceding.

1. Thus, one grain in two ounces of serum gave a Prussian blue.
2. Half a grain a bluish green.
3. Quarter of a grain gave a decided green with a shade of blue, but not so deep, or characterized, as in the experiment on the serum of the second dog. Whence we may infer, that in the serum of this dog there was, at least, one eighth of a grain of the hydrocyanate to the half ounce.
4. One eighth: a couple of drops of the iron test liquor coagulated the serum, as it did indeed in every trial, and gave an olive green, with a distinguishable shade of blue, but still lighter than in No. 3.
5. One sixteenth: the addition of the test did not alter the light yellowish brown colour of the serum so much as in the former experiments, in which it was turned more or less green, but there was a distinguishable shade of blue, especially where the mixture lay thin on the side of the tube and light was transmitted.

In all these experiments the production of the green colour must be ascribed in part to the effect of the test on the caustic alkali of the serum; for I have frequently seen that a shade of green results from the addition of a solution of permuriate of iron to that of caustic potash or soda in water.

Not knowing what salt of iron was employed by Sir Everard Home in his experiment above referred to, I went over my experiments on serum with a saturated solution of crytallized proto-sulphate of iron in distilled water.

It gave with the serum No. 1, a *bluish* green, in which the blue predominated under the direct rays of the sun and reflecting them.

With No. 2 a *greenish* blue; meaning that the green predominated, seen in the same light.

With No. 3, a pale green and no perceptible blue.

With No. 4, the colour of the serum seemed only to be diluted. There was no appearance of green or blue; which renders it probable that wheresoever green was formed there must have been present more or less of the blue colouring matter, and consequently, that it was not wanting even in the serum of the blood of the first dog, since this also gave a marked green with the permuriate of iron.

It also results, that the permuriate of iron is more sensible than the proto-sulphate, in the proportion, at least, of 1 to 16.

The chyle, which Professor Chapman considers next to the blood in point of elaboration, was not in those cases the same as if no hydrocyanate of potassa had been taken, and consequently it disproves the assertion, that *the process of assimilation completely animalizes all articles subjected to its influence.* "Can it be credited, is the question asked, that any substance, after a subjection to the digestive and assimilative powers, retains in the slightest degree its original properties.*" The experiment just detailed will certainly warrant a belief that it may; at least as relates to the chyle, in which the hydrocyanate of potassa was apparent, and not decomposed, nor reduced to a homogeneous fluid.

If, according to the opinion of Professor Chapman, the process of assimilation as performed by the chylopoietic viscera, or by any part of the absorbent apparatus, completely decomposes all substances, and, however discrepant in their properties, reduces them to a homogeneous fluid, fitted for the purpose of nutrition; if this opinion be a just interpretation of the laws of the animal economy, medicines themselves, when taken into the stomach, are successively converted into nutriment; and being thus reduced to the condition of common food, can have no other, or greater effects than our aliments.

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* Chapman's Therapeutics, vol. i. p. 43. 45.

If mercury and bread be alike convertible into nutritious matter, without the least difference, why is mercury more requisite than bread for the cure of any disease? Or what necessity is there for any of the substances called medicines? seeing that their properties begin to vanish from the moment of their reception by the stomach, to that of their expulsion from the body.

In the way in which I understand the doctrine of Professor Chapman, the solution of this difficulty might be attempted by saying that the moment medicines touch the stomach they excite in it a sympathetic action ere they are altered into chyme, and that this action constitutes their curative power. But though the gratuitous assumption were allowed, it still remains as a difficulty that the change, or metamorphosis, of the medicine begins the moment it is taken, and is consummated as soon as chyme and chyle are formed. Medicines, it must then be owned, will have a very short time allowed them for effecting their cures: and to accomplish any thing they have need to be instantaneous in their operation. This would just leave us a Brunonian Materia Medica, and limit our remedies to the diffusible stimuli alone.

We know, for a fact, that substances which are insoluble do not affect the constitution. The mechanical results of one or two drugs cannot be deemed an exception. And if they are inactive, as medicines, which cannot mingle with our fluids, certainly they can be as little active, which are entirely altered, digested, assimilated, converted into bland nourishment. Now, whereas medicines do act, and even some very slowly, it follows necessarily, we think, that they are not entirely altered, but enter the system with enough of their own nature left to produce their peculiar effects.

There seems to be a contradiction in maintaining that not a vestige of the original properties of a drug is left, and yet, that it can act as a medicine. When a dose of salts, for instance, produces the *sympathy* of purging, they are not yet assimilated, but as long as they evacuate, they retain their proper nature throughout the intestine, and may be detected in the feces.

I have ascertained, that after taking one ounce and a half of phosphate of soda, if the feces of the second or third discharge be filtered and treated with muriate of lime, a precipitate will be left, having the characters of phosphate of lime.

Aloes taken instead of salts exerts no purgative sympathy on the stomach. It retains it all, in spite of the assimilating powers of the stomach, duodenum, pancreas, and liver, until it reaches the large intestines, on which it acts with its own affinity to those parts, and from which it passes off in the feces.

If mercury be rubbed on the thighs it will salivate the mouth; but cannot, I should think, be assimilated to our food while it is producing salivation. If it did not retain, in the slightest degree, its original properties, how could it cure syphilis? In the case supposed, should it be said that it produces its effect by an impression on the skin, or on the internal solids, such as the salivary glands, after absorption; even so, it must arrive at those glands in a condition to stimulate them. It must arrive, not assimilated, digested, and altered, but as an active mercurial preparation. The idea of a medicine suggests a substance tenacious of its nature, and that resists assimilation and change before it reaches the organ to which it has an affinity, and on which it produces its appropriate effect. With regard to mercury those organs are the glands, not the salivary glands only, but all; the liver among others, in the diseases of which, mercury is so often serviceable by restoring or increasing its secretions. Wherever rubbed in, the mercurial ointment is carried by the circulation to the glands, which are then excited by the fluids thus charged with the mercurial preparation. We mean that they are the vehicle of the remedy, and it follows that they may as well be the vehicle of disease. The sanative action, in the present instance, depends on the mercurialized fluids, and consequently on their composition; but when it is once admitted the source of action may be in the fluids, we cannot deny that they will produce a morbid one when they are disordered, which is the important conclusion.

As to the fact, that substances introduced into the circulation act upon some organs more manifestly than upon others, by what I may be allowed to call a species of affinity, it rests on manifold experiments, and cannot fairly be questioned. Those substances circulate, consequently, with the blood, unassimilated and unchanged. A moderate dose of a solution of tartar emetic transfused into a vein has produced vomiting, and that alone. A large dose of infusion of colchicum transfused by Sir Everard Home into the vein of a dog, produced inflammation of the intestines, as if he had given it by the mouth. The tartar emetic circulated with the blood, but only acted on the organ best adapted to feel its impression, in consequence of that system of sensibilities by which light stimulates the eye, and air the lungs.

It being undeniable, that saline bodies taken by the stomach are discovered in the secretions unchanged, the eloquent advocate of the exclusive pathology of the solids, while he advances that the process of assimilation completely animalizes all articles subjected to its influence, alleges, at the same time, "that the secretions and excretions being removed beyond the sphere of the vital energies, the chemical affinities are sometimes again brought into play, by which substances (previously decomposed, &c.) are in part or wholly regenerated."*

Of this allegation there is offered no proof; so that it rests upon its own inherent credibility, or coincidence with other phenomena. Now, if we find certain substances in the serum of the blood and in the secretions that minister to chylification, which we also find in the excrementitious secretions of perspiration and urine, they may certainly exist in every fluid, and there is no sound reason for supposing that they are decomposed in one place to be reproduced in another.

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* Chapman's Therapeutics, p. 45.

In the serum of the blood we find,

Soda,
Muriate of potash and soda,
Phosphate of soda,
Phosphate of lime and magnesia.

In the colouring matter of the blood we find,

Phosphate of lime and magnesia,
Lime,
Carbonic acid,
Oxide of iron.

In the bile we find,

Soda,
Phosphate of soda,
Muriate of potash and soda,
Phosphate of lime and magnesia
Oxide of iron.

In the urine we find

Carbonic acid,
Phosphate of lime and magnesia,
Phosphate of soda,
Muriate of soda,

In the perspiration we find

Carbonic acid,
Phosphoric acid,
Muriate of soda,
And other saline bodies.

We here see that fluids, which differ greatly in composition and properties may nevertheless contain many substances in common, and that these are to be found every where from the stomach to the bladder.

It is true the humoral pathology derives strength from such facts, for they show that several heterogeneous bodies may exist, without alteration of their properties, in the human fluids: and when a gentleman of distinguished talents and learning, in order to escape from the pressing consequences of such facts, can do nothing better than imagine an ingenious but unsupported hypothesis to account for them, we may safely conclude that the doctrine which totally rejects the pathology of the fluids is itself untenable. Indeed, it is more consonant to sound reason that the causes of our well-being and infirmities should exist in all parts of our complicated existence than in any one division of them exclusively.

A thing that has long influenced the theories of medicine is the importance attached to the study of chemistry. At one time it was exaggerated, at another no less undervalued; and even as yet the schools of medicine have not entirely devested themselves of prejudice on the subject. Those teachers of medecine who make light of chemistry disregard the fluids in their pathology, and are all for sympathy and irritation. They, on the contrary, who are well acquainted with chemistry, and, consequently, with the composition of the fluids, cannot believe but their difference must have its effects. Possibly the ardour with which chemical studies are more than ever prosecuted in Europe may be one cause, after a long interruption, of the growing estimation of the humoral pathology in that scientific portion of the world.

Art. II. A Sketch *of the Legislative provision of the Colony and State of New-York, respecting the Practice of Physic and Surgery, communicated to the Editors by* T. Romeyn Beck, M. D., *Professor of the Theory and Practice of Physic and of Medical Jurisprudence in the University of the State of New-York.*

The following observations were originally intended to form part of an Essay on the progress and present state of learning in the State of New-York, but as the period is uncertain, when I shall be enabled to complete it, I ask the favour of the insertion of what relates to Medicine in your Journal.

An extended history of medicine applicable to our state and country, should, I apprehend, contain a notice of the following particulars: 1 A sketch of the origin and progress of medical schools. 2. The provisions which the government has from time to time made to improve the profession. And 3. Biographical notices of distinguished physicians and surgeons. The first of these has already received considerable attention, and the last must be the work of many hands. I shall therefore confine myself to a notice of the second division.

The first colonial assembly of the province of New-York met in the year 1683; and the earliest record of legislative acts is, I believe, contained in a volume entitled "Laws of her Majesty's Colony of New-York, enacted in various sessions, the first of which began April 9, 1691, down to May 24, 1709," printed by William Bradford, 1710. After a careful examination of this work, I have found but one section that relates to the medical profession. It is interesting, inasmuch as it exhibits a difference of opinion between former legislators, and those of more modern times on a much disputed point. The section, or rather the proviso in question, is to be found in the law entitled "An act for settling the militia," passed in 1709. After enumerating those who are liable to be enrolled, it proceeds to state, that all the members of her Majesty's council, justices of the peace, high sheriffs, coroners, and all officers of courts, ministers, schoolmasters, and *physicians and chyrurgeons*, shall be free from being listed in any troop or company within the province.

The law regulating the militia of the province of New-York was continued with various amendments and alterations until the period of the American Revolution. In all the various acts, a proviso similar to the one stated above, is contained, and it is to be found in the one passed March 24th, 1772, the last of this nature that was adopted previous to the commencement of hostilities.*

Physicians and surgeons were, however, liable to perform professional duty in cases of invasion, or even when it was found necessary, in self defence, to attack a neighbouring province. In the act authorizing the raising of two thousand six hundred and eighty men, being the quota of the province in the force of twenty thousand men, which were to invade Canada in 1758, it is ordained, "that in case physicians or surgeons, or their assistants, being appointed by the governor to attend said forces, shall refuse, delay, or neglect to go on said service, they shall individually forfeit the sum of £100."†

The foregoing extracts are all that I have been enabled to discover, relative to the medical profession, after a diligent examination of the colonial laws from 1691 to 1773, with the exception of the following. As it is the first law on our records in which a legislative attempt was made to regulate the practice of physic and surgery, it deserves particular attention.

The act is entitled "*An act to regulate the practice of physick and surgery in the City of New-York.*" It was passed in June, 1760, and the preamble gives the following exposition of the state of the profession. "Whereas many ignorant and unskilful persons, in physic and surgery, in order to gain a subsistence, do take upon themselves to administer physick and practice surgery in the city of New-York, to the endangering of the lives and limbs of their patients, and many poor and ignorant persons, who have been persuaded to become their patients, have been great sufferers thereby,"—it is therefore enacted, that after the publication of this act, no person whatsoever shall practice as a physician or surgeon in the city of New-York, before he shall have been first examined in physick and surgery, and approved of and admitted by one of his Majesty's council, the Judges of the Supreme Court, the Attorney General, and the Mayor for the time being, or by any three or more of them, taking to their assistance, for such examination, such person or persons, as they in their discretion shall think fit. If the candidate be approved, a certificate shall be given, signed by the examiners, allowing him to practise physic or surgery, or both, throughout the province of New-York.

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* Laws of New-York from 1691 to 1773, Revised by Peter Van Schaack 1774, p. 672.

† Laws of New-York, from 1752 to 1762, digested by William Livingston and William Smith, junr.—1762. vol. 2. p. 123.

The second section establishes a penalty of five pounds, with costs, against any person found practising without the above testimonial.*

The necessity of such a law must have been most evident, when we find what language the grave historian of New-York employs on this subject. "Few physicians," he observes, amongst us are eminent for their skill. Quacks abound like locusts in Egypt, and too many have recommended themselves to a full practice and profitable subsistence. This is the less to be wondered at, as the profession is under no kind of regulation. Loud as the call is, to our shame be it remembered, we have no law to protect the lives of the king's subjects from the malpractice of pretenders. Any man, at his pleasure, sets up for physician, apothecary, and chirurgeon. No candidates are either examined or licensed, or even sworn to fair practice."† It appears, by the dedication to this work, that it was published in 1756. The law regulating the practice was, as I have already stated, passed in 1760.

After the revolution, the legislature applied themselves with assiduity to the revision and enacting of laws, which the change from a dependent colony to an independent state required. The act to regulate the militia was revised in 1786, and in the third section, it was ordained, that physicians and surgeons, except in their respective professions and callings, should be exempted from military duty.* A similar provision is to be found in the acts of 1783 and 1801. In the session of 1809, it was, however deemed proper to omit this clause, and physicians and surgeons became in consequence liable to perform military duty. As an uncuccessful attempt was made in 1810, to re-insert the exemption, and as the militia law has remained unaltered in that respect, until the present time, we may consider it probable that no change will be made. The impropriety, and indeed injustice of this requisition, is so self evident, that I need not enlarge on it. As far as my information extends, New York is the first civilized state, either in this country or in Europe, that has demanded military duty from the medical profession.

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* Livingston and Smith, vol. 2. p. 198. The act, however, exempted from its operation all persons *already* practising medicine in the city.

† Smith's History of New-York, 1814, p. 326.

The first act to regulate the medical profession was passed in March, 1792. In its operation, it is confined, like the one of 1760, to the city and county of New-York. Its preamble gives us also the same melancholy picture of the state of medical practice "*Many ignorant and unskilful persons presume to practice physic and surgery within the city and county of New-York to the detriment and hazard of the lives and limbs of the citizens thereof.*' The requisitions of this law are of a higher cast than those o: the previous one; and indeed the progress of improvement in this respect may be dated from the above year. They require 1. Studying with some reputable physician for the term of two years, if the individual has graduated at some College in the United States, or three years, if he has not. 2. An examination before the governor, chancellor, the judges of the supreme court, the attorney general, the mayor and recorder of the city of New-York, or any two of them, who are to take to their assistance any three respectable practioners, *with whom the examined person has not lived.* The certificate of this board constituted the license to practice, and no individual without these testimonials was allowed to have a legal demand for his services. This law, however, declares that its provisions are not to affect those who are already in practice; nor to prevent physicians from neighbouring states or counties being called in consultation; nor finally to effect those who have regularly obtained the degree of Doctor of Medicine.*

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* Jones and Varick's Edition of the Laws, 1789, vol. 1. p. 266.

The above provisions, it will be observed, relate exclusively to the city and county of New-York. The progress of population and the wants of the community rendered it necessary, in a short time, to form regulations which should be binding and operative on the whole state. Accordingly, on the 23d of March, 1797, the first general act was passed, "*to regulate the practice of physic and surgery in the state of New-York.*† This document by implication explains some of the evils under which the state was labouring in consequence of the unbounded privilege, which all enjoyed of styling themselves medical practioners.

The first section is retrospective, and examines into the right of the individual to assume the title in question. It ordains, that from and after the 1st of October, 1797, no person *now practising physic and surgery, or either*, shall be allowed to continue to do so, unless he shall have produced satisfactory evidence, to one or other of the high officers enumerated in a former act, that he has practised physic and surgery, for the term of *two years next preceding the day aforesaid*, or shall in a similar manner, exhibit evidence of having studied medicine for the term of two years. Either of the magistrates mentioned above was authorized to grant him a certificate, and this was to constitute his license as he is directed to file it in the office of the clerk of the county where he resides; and the penalty for practising without such a filed certificate was declared to be twenty five dollars.

The second section relates to the student. After the same date, he was not allowed to practice, unless he exhibited to either of the above magistrates, a certificate from one or more reputable physicians or surgeons, with whom he had served his apprenticeship, stating that he had studied the profession for *the term of four years*, and that he was well qualified. The persons signing the certificate were to make oath to the same, and the magistrate was then to endorse on it a permit allowing the individual to practice. This was also to be filed in the Clerk's office. Graduates of Colleges were, however, allowed to complete their term of study in three years; and, again, all those who received the degrees of M. B. or M. D. from a medical institution were allowed to practice, on filing their diplomas. One or two clauses, are also added, directing the mode of proceeding, in case the physician signing the certificate was dead or non-resident. Proof was in such cases to be taken, and if it satisfied the magistrate, he was authorized to give the necessary permit: and, as the operation of this law was co-extensive with the state, the act of 1792 was repealed.

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* Greenleaf's Edition of the laws. vol. 2. p. 425.

† Greenleaf vol. 3 page 417.

This law certainly reflects great honour on the framers of it. Its natural effect must have been to exclude many ignorant and presuming pretenders from the privilege of practising in a licentious manner; and, at the same time, it laid the foundation for an improved race of junior physicians, who from the increased period of study, and the duties incident thereto, were calculated to exalt the standard of medical character. It continued in operation for several years; and the amendments to it do not materially affect its general outlines. In 1801, when the laws of the State were revised under the care of Judges Kent and Radcliff, the act was re-enacted, without alteration, except that permission was given to a practitioner removing from a neighbouring State into this, to practice three months without a permit; probably to allow him time to procure the necessary certificate.* The first section, however, seems not to have been acceptable to *some practitioners*, since in 1803 a law was passed, relating to those who had been in practice before the 1st of March, 1797, which extended the time for filing the proof of the period of study until January 1st, 1804.* By the previous act, it was restricted to the 1st of October, 1797 This was certainly a liberal provision on the part of the legislature. The unfortunate individual who was *surprised* by the appearance of the law. which questioned his right to exercise a responsible profession, was allowed nearly *seven years*, to show that he had properly qualified himself An indefinite extension would have better suited the cases of several.

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* Kent and Radcliff's Revised Laws, 1822, vol. 1. p. 449.

There are two or three laws which may be noticed at this time, from their connection with the subject in question. They relate to individuals, but notwithstanding deserve attention on many accounts.

The act of 1797 pressed hard on an individual named William Firly, a resident of Suffolk county. It appeared by a petition of the inhabitants of Brookhaven and other towns in that county, that he had performed several cures, particularly of scorbutic diseases. His situation, however, prevented him from obtaining a certificate, according to law, and they prayed for relief. The legislature listened to the petition, and directed the first judge of the county, (in case three reputable physicians practising there should state that he was capable and qualified) to grant him a certificate.† This is the only instance on record, where the legislature have exercised their plenary powers in modifying the law of 1797, for the relief of an individual.

In the session of 1806, a law was passed. allowing a compensation to a somewhat notorious personage, named John M. Crous, for discovering and publishing a cure for hydrophobia. The sum of one thousand dollars was granted to him, on the condition, however, that if his remedy should, within the term of four years, prove deceptive and ineffectual, the above sum with interest should be refunded. The prescription was also ordered to be published throughout the state.‡

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* Webster's Editions of the laws from 1802 to 1812, vol. 3. p. 334.

† Webster, vol. 3. p. 639.

‡ Webster, vol. 4. p. 348.

Although the individual and the legislature in question have been equally the subject of ridicule, yet it can hardly be questioned, I apprehend, that this proceeding was praise-worthy, and deserving of general imitation. Canine madness is a disease of the most dreadful nature. It would appear that human foresight can hardly guard against it. All and every one of the community are alike exposed to it. Is it not then a paternal act to throw open to the public, every possible avenue of escape from the awful termination that awaits the diseased?

It is well known, that after divesting the prescription of its quackery, there remains an important remedy, whose effects on the human system are not yet fully understood. I refer to the *acetate of copper.*[*]

I regret that I cannot equally commend an act of a subsequent legislature, by which they permitted "Peter Dawson, *for ten years*, to use and improve the skill he *possesses* in curing the malady called cancer, and directing that whenever he is employed for that purpose, he shall be entitled to recover fees for his services."[†] It is well understood that the active ingredient used by this individual, is a preparation of arsenic, and it is applied externally to the diseased part. The medicine itself is said to be patented. As to the originality of this remedy, a glance into Paris' Pharmacologia will be sufficient to convince the reader that it is destitute of foundation.[‡] As to its efficacy, and even the safety of its application in many cases, let the experience of the medical public decide.

The law which regulated the medical profession from 1797 to 1806, enabled the student to commence practice on the certificate of his preceptors. No examination was required, and in this respect, indeed, the act in question was not so useful as that of 1792. We are now, however, approaching a period when the system of examination became a part of our statutes. In 1806 a law was passed, *to incorporate medical societies, for the purpose of regulating the practice of Physic and Surgery.*[*] As this is the basis of the present Medical Code in this state, it deserves particular attention. I shall endeavour to condense its provisions, and, at the same time, omit such as appear foreign to the object in view.

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[*] See some judicious remarks on the subject by Dr. Hosack in the American Med. and Philos. Register, vol. 1. p. 462.

[†] See the law in the Secretary of State's office, passed March 30th, 1809. It has never, I believe, been published except in the State Papers.

[‡] Page 209. Can it be that the prescription of Davidson, mentioned in the note to this page, is the one in use here? and has this nostrum travelled to London?—"*Arsenious acid and powdered hemlock!!*"

It directs that the physicians and surgeons in the several counties of this state, *now authorized by law to practice,* shall meet on the first Tuesday of the ensuing July, and there, if their number was not less than five, form themselves into a medical society. These societies were again, by delegates, to constisute a state medical society, which was to meet annually at the capitol of the state.

These corporate bodies were authorized to appoint censors, whose duty it was to examine all students presenting themselves. They were thus made the sole power by which licenses to practice could be obtained, and the law declared, that all who attempted to practise without such law or diploma should be disqualified from collecting any debts on that account. The student was to exhibit testimonials of his having studied medicine for the term of *three years,* before the censors were allowed to commence the examination,[†] but always proceeding on the principle of improving the profession in future, and not pressing too hard on the peculiar circumstances of those now in practice or preparing, the law also allowed such as had commenced their studies prior to the first of January, 1805, to study *four* years, and then to be licensed according to the law of 1797, or rather of 1801. And that law was declared to be repealed on the 1st of September, 1806, at which time the present act was to go into operation. As to physicians coming from any other state or country, it declared with a spirit of liberality, that all persons duly authorized to practise by the law of such state or country, having a diploma from a regular medical society, should be allowed to practise in this state without molestation.

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[*] Webster, vol. iv. p. 537.

[†] In case a student considered himself aggrieved by the decision of a county medical society, he was allowed to appeal for examination to the State Medical Society.

Various amendments to this law were from time to time, found necessary. In April, 1807, an act was passed which contains several clauses, regulating the internal organization of the medical society of the state. It then ordains, that the penalty for practising without being authorized according to the above act, should be *five dollars per month.* This, however, was not to prevent a person from using or applying for the benefit of the sick, any roots or herbs, the growth of the United States.* In March, 1809, it was enacted, that the degree of Doctor of Medicine granted by the Regents of the University, should allow the person to practise physic and surgery—any law to the contrary notwithstanding.† Again, in May, 1812, the penalty for practising without license was increased to twenty-five dollars for each offence, and licenses are directed to be deposited in future in the Clerk's office.‡

On the revision of the laws in 1813, the above acts of 1806, 1807, 1809, and 1812, were consolidated, with the exception of those provisions which had become obsolete or nugatory. Accordingly, the revised law, passed April 10, 1813,§ is the authority to which physicians are to refer in all disputed cases connected with this subject.

In 1818 the legislature superadded to the requisites already in force, some additional ones. By the act of 1813, the student was required to study medicine three years, and at the end of that time, submit to an examination. By the present act, it was declared, that from and after the 1st of January, 1821, he should study *four years*, with, however, the following exceptions: One year might be deducted from the above, provided he had pursued classical studies for that period, after the age of sixteen; again, "*attendance on a complete course of lec-*

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* Webster, vol. 5. p. 114.

† Webster, vol. 5. p. 565.

‡ Webster, vol. 6. p. 396.

§ Van Ness and Woodworth's Revised Laws, vol. 2. p. 219.

tures delivered by each of the professors, on all the branches o medical science, in either of the medical colleges or institutions in this state or elsewhere," might be accepted in lieu of one year's study. The examinations were to be continued, but no person was to be licensed, until he shall be of the age of twenty-one years.

It had also happened that students who were rejected by one county society, had presented themselves, and been admitted to practise, by another. This was now prevented by totally forbidding such a course of conduct, and an appeal to the State Medical Society was alone allowed. Licenses obtained in contravention of this provision are declared void.

By a reference to the law of 1813, it will be seen that the requisites demanded of a physician or surgeon coming from another state or country, are expressed in a vague manner. These were now defined with precision. A foreign practitioner is not allowed to practise in this state, until he shall have filed a copy of his diploma, from some college of medicine or legally incorporated medical society, with the clerk of the county, in which he resides, nor until he exhibits to the county medical society satisfactory proofs that he has studied medicine agreeable to the requisitions of this law.

The above extracts from the statutes, which have been in force at different times in this state, will give a fair view, I believe, of what has been done by our local government for the improvement of the profession generally. It will be seen that the requisites necessary for practising medicine have gradually been increased, until we arrive at the laws of 1813 and 1818. A definite period of study, and examination at the end of that period are demanded, while attendance on a full course of medical lectures is urged by a consideration which must always have great weight with the youth of our country, who have to be the artificers of their own fortunes.

I have omitted a notice of the regulations contained in these statutes respecting medical societies and the medical colleges, as they do not come within the scope of this essay. For the same reason, animadversions on various provisions in the existing laws, as well as suggestions for further improvement are also waived. This last is an inexhaustible subject, and deserves separate notice. My object has been to give a plain statement of what has been done—of *what is actually the law in this state*; and if these observations should by accident fall into the hands of a British practitioner, to show him how fallacious is that authority which informs him, that in the practice of physic, surgery, and pharmacy, in this country, "*no preparatory course of study, no testimonial of competency, no kind of examination, no particular qualifications, no diploma or license are required.*"*

Albany, May 1, 1822.

ART. III. *Case of* UTERINE HYDATIDS: *Communicated to the Editors, by* WILLIAM MOORE, M. D. *New-York.*

Mrs. G.——— had missed her courses for two months, when she weaned her child supposing herself to be pregnant, as she had been very regular for some months previous, notwithstanding her having given suck. She was then seized with a flooding accompanied with some pain, and expected that an abortion was about to take place. The pains, however, soon abated, but the discharge of blood continued for six weeks, during which time she lost a great quantity of blood, and her strength was very much reduced. The uterus continued to increase in size all this time, notwithstanding the discharge of blood, for it was as large as in a woman four months advanced in pregnancy; it also felt hard, and she complained that it was sore to the touch, when pressed upon above the pubis. She was now seized with severe labour pains, and the flooding was greatly increased, so much so, that she fainted and appeared to be in danger of sinking under it. The usual remedies to restrain uterine hœmorrhage were used, but with little effect.

As she had endured a number of severe pains, it was thought advisable to ascertain the state of the mouth of the womb. Upon introducing the finger into the vagina, it was found to be filled with coagula, and the os uteri was somewhat dilated with something hanging out of it, which felt different from coagulum, but what it was I could not ascertain; I therefore determined to introduce my hand into the vagina, and make an attempt to empty the womb, by bringing away the substance, whatever it might be, which was protruding from its mouth. When I passed my hand into the vagina, it pressed away a large quantity of coagula; and taking hold of the substance at the mouth of the womb, I brought away a cluster of hydatids. The os uteri was sufficiently dilated to admit two fingers: I introduced the forefinger into the cavity of the uterus, and found it filled with the same kind of substance; I broke it up with my finger and brought away another considerable portion.

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* See Quarterly Review, vol. 10. p. 509. American Edition, *Review of Inchiquin's Letters.*

The pain and irritation, occasioned by introducing the hand into the vagina, increased the force of the pains and moderated the flooding; it was, therefore, determined to leave the emptying of the uterus to the labour pains, which in the course of three hours forced away about four pounds of this curious substance. It consisted of clusters of transparent globules, about a quarter of an inch in diameter, and containing a colourless fluid. These globules were enveloped in a semitransparent gelatinous substance of a reddish cast, approaching somewhat to a flesh colour, and of considerable tenacity. As soon as the uterus was emptied the pains ceased, the flooding abated, the discharge stopped gradually, and the woman, by slow degrees, recovered her former health.

This case was the first of the kind I had ever seen. It occurred in September, 1797. Since that time I have met with but one other case, which happened in October, 1804, to a Mrs. W———, whose symptoms were very similar to those of the above case, and the event was equally fortunate, for the lady recovered perfectly, and had several children afterwards.

Art. IV. History *of the* Yellow Fever *as it occured at Knowle's Landing, Connecticut, in a letter to* David Hosack, M. D. *By* William Tully M. D. *of Middletown, Connecticut.*

Middletown, (Con.) February 1st, 1822.

Sir,—In large maritime cities, where there is free and constant intercourse with foreign ports, where of necessity there are great accumulations of filth from various causes, and where so small a portion of the inhabitants are personally acquainted, it must always be attended with considerable difficulty to ascertain, with precision, what pestilential diseases are of foreign origin, and what are domestic. But on the other hand, in small towns, and country villages, where there is but little intercourse with strangers, where there are no local causes of disease, and where the inhabitants are all known to each other, and acquainted with each other's concerns, there can be no difficulty in settling questions of this sort, with sufficient certainty to satisfy every rational mind. Indeed, under such circumstances, there can be no variation in sentiment, except with the few, who imagine that they cannot be independent, unless they differ in opinion from the great majority of men of common sense. With such persons arguments are useless, and the strongest proof produces no conviction.

Under these impressions, I have always believed, that the casual occurrence of yellow fever in the small and healthy ports of New-England, particularly of those on the river Connecticut, afford the most incontrovertible evidence of its foreign origin that is perhaps to be found in the whole world. In these places, intermittents and bilious remittents, the endemics of southern climates, are unknown, except in a few instances, in persons who have recently arrived from places where such diseases prevail. In short, we have no disease that could possibly be suspected of being yellow fever except a few sporadic cases of putrid fever, which none could mistake but the superficial, or those whose theoretical notions lead to such a generalization as confounds all distinctions of species in disease. The able report of Dr. Beck, upon the fever in

Middletown, in 1820, to the accuracy and fidelity of which upon every essential point, I am happy to bear the fullest testimony, it is believed, will completely justify the foregoing remarks. An account much more in detail, and which is thought to contain some additional evidence upon the subject, has long been prepared for the press, and will, one day, be given to the public. Upon this topic, I have now only to add, that at least as many cases traceable to the same vessel occurred elsewhere, the same season, as at Middletown.

I have been lately led to investigate the circumstances of the Yellow Fever in a neighbouring town, upon this river, which give additional confirmation upon this point. Among those at all acquainted with the facts, there was never a question respecting its origin.

As this is a subject upon which you, sir, are publicly known to have taken so much interest, and which you have contributed so much to elucidate, I have concluded to address the following statement to you, entirely submitting its *final disposal* to your discretion. I do this under the impression that the facts which ensue have never been published, as I have not noticed any reference to them in the controversy upon the great question of the origin of the yellow fever.

The village of Knowle's Landing, a part of the parish of Middle Haddam in the town of Chatham, is situated on an uneven, irregular, and rather steep declivity, on the east bank of the Connecticut, about six miles below Middletown. At the period referred to, it must have contained about thirty houses, and nearly two hundred inhabitants, and was then, as it now is, remarkable for its salubrity and exemption both from endemic and epidemic diseases. At that time, its inhabitants usually employed two or three vessels in the West-India trade.

In the month of August, 1796, during the prevalence of the usual general health in the village, the brig Polly, Russel Doan master, owned by Bulkley and Russel Doan, arrived from Cape St. Nicholas Mole. Her outward cargo consisted of oats and other grain, which was sold for cash, so that she returned merely in ballast, and in as cleanly a state as such vessels usually are after such a voyage, On her homeward passage, one of her crew, by the name of Mayhew Tupper, died on board, with Yellow Fever. The clothes which he wore, while sick, were thrown overboard, though a sail upon which he lay, when he died, was retained. The vessel subsequently underwent a sailor's washing, which in this instance consisted in scrubbing with cold water and the application of vinegar. On the arrival of the Brig at this Landing, Newel Hurd, a young man of about eighteen, and John Ranney, were employed to assist in clearing her out. They were known to handle the sail on which Tupper died. At the same time, Sarah Exton, wife of one of the crew, and Elizabeth Cook, who lived in the same house, were employed in washing some of the sailors' clothes. A few days afterwards, these four persons were attacked with yellow fever. In about five days, viz. on the 29th of August, 1796, Hurd died, and within twelve hours, Ranney, and Sarah Exton. The alarm in the village was already so great that Sarah Exton was left alone in the night, and was found dead in the morning, with her infant child at the breast. Elizabeth Cook was soon added to the number of victims, and in quick succession, the infant child of Sarah Exton, Lucinda Norton, a tailoress, a member of the same family, who had been employed in repairing some of the sailor's clothes, and Elizabeth and Rebecca Carey, daughters of Elizabeth Cook, were taken sick, and survived but a few days.

The whole of these cases commenced and terminated within the period of a fortnight. Some time during their existence Gersham Lord, who acted in the capacity of clerk to one of the owners of the vessel, and who had been on board of her, but had no intercourse with the sick on shore, was taken with the same disease, and after a short, but severe illness, recovered. Beside this, there were two or three other imaginary, or at most, light cases, among those who had intercourse with the vessel or the sick, which soon recovered.

Some time while the brig was lying at this Landing, but whether during the existence of any of the other cases, or subsequently, I have not ascertained, two sons of William Dixon, Esq. of the town of Chatham, about seven miles above, passing along the river in a boat, in the night, hauled along side the brig, and slept till morning in the cabin. Soon after they reached their father's both sickened with yellow fever. One died, but the other recovered.

So unaccustomed were the inhabitants to such a rapid and fatal disease, that the whole village was panic struck. After the three first deaths, Dr. Bradford, an old physician, resident in the place, and Drs. Hollister and Thacher, two young men who were candidates for business, departed precipitately, and did not return till all traces of the disease had disappeared; and so many of their customary employers followed their example, that only five persons had firmness and humanity sufficient to remain, and take care of the sick, and bury the dead.

To the principal individual of this small band of philanthropists, Ralph Smith, Esq., I am chiefly indebted for the foregoing facts. They have, however, been confirmed in detail, by Mr. Nathaniel Doan, brother, as I believe, to one of the owners of the brig.

I do not attempt to particularize the symptoms of the disease, as there was never the least question with respect to its nature. Mr. Smith had frequently been in the West-Indies, and had had opportunity to see much of yellow fever; and there were other seafaring men who were able to identify it with the scourge of the tropics.

The physicians who attended the latter cases, were Dr. W. B. Hall of Middletown, and Dr. J. Richmond of a neighbouring parish in Chatham. They are neither of them now living; but it is universally known that they had no doubts upon the subject.

It ought to be observed, that though the subsequent autumn and winter were uncommonly dry, yet there was nothing peculiar in the weather of August. Perhaps the month might have been considered as rather warmer and drier than usual, but it was by no means prominently so. It was, on the whole, one of our finest seasons, and the health of the adjacent country was as good as it ever is. Simple Fevers, Dysenteries, and Choleras did not prevail at all.

Thus it appears, that from this single vessel, there originated, unequivocally, eleven cases of yellow fever in the town of Chatham, nine of which died—as adequate a number to prove the importation of the disease, as if it had been eleven thousand—a fact, in this point of view, as irresistible as any on the records of medicine.

I have only to add, that there has been scarcely a season, for the last five-and-twenty years, in which individual instances of yellow fever have not occurred on the river, though the greatest number that has happened, at one time, has been at Hartford, at Middletown, and at Knowle's Landing; and with the exception of a few theorists, there never has been a doubt of its foreign origin. *With us* it is, usually, even much more difficult to trace the small pox to any evident source than it has hitherto been to account, satisfactorily, for the introduction of yellow fever. It will, of course, be understood, that I do not mean to include in this remark, those sporadic cases of disease which are occasionally produced, in particular seasons, by foul cellars, and decaying vegetables, especially by putrid cabbages. That these instances are as malignant, as rapid, and as deadly as yellow fever, I make not the least question; and, if we may credit reports, have been sometimes attended with a yellow skin, but are, nevertheless,? upon every principle of discrimination adopted in other cases, to be considered as essentially distinct, as they lack all the true pathognomonic symptoms, and are, in all probability, only more violent cases of genuine putrid fever.

In the foregoing, it will be noticed, that there is nothing said upon the subject of the communication of the disease from one individual to another on shore. In this part of the country such an occurrence is extremely rare. No decided instance of this sort has ever fallen under *my* observation, except in Middletown, in 1820. It is believed that the cases at Knowle's Landing are all to be accounted for in some other way, except perhaps that of the infant child.

I cannot forbear adding one circumstance still further, which tends to show the salubrity of the situations, and the great degree of health of the towns upon the river Connecticut, which is the well known fact, that of all that are born, one sixth, or at the very farthest, one seventh survive the age of seventy years.

Yours sir, very respectfully,
David Hosack, M. D. WILLIAM TULLY.

Art. V. A Report *to the New-York Board of Health, concerning the nature and origin of the Malignant Fever in Middletown, Connecticut. Presented on the* 17*th of July*, 1820. *By* John B. Beck, M. D.

In compliance with the instructions received from the New-York Board of Health, to proceed to Middletown, Connecticut, for the purpose of inquiring into the nature and origin of the fever which had been reported to prevail in that city, I left New-York by the first conveyance after my appointment, and reached Middletown on the 5th of July. As soon as possible after my arrival, I presented to his honour the Mayor, a letter addressed to him by Mr. Jay, the President of the New-York Board, and which informed him of the object of my visit to Middletown. He had no sooner read the letter, than he communicated to me, in the most unreserved and candid manner, every fact in his possession which could tend to aid me in accomplishing the designs of my mission, adding at the same time, that neither himself nor the Board of Health of Middletown, wished to conceal any thing that might inform the public of the actual state of the health of the city. In addition to this, I held interviews with all the physicians of the city, who had attended upon the different cases of fever. From these sources I obtained most of the facts embraced in the subsequent report.

In a former communication to the Board, I stated that thirteen cases of malignant fever had been reported to the Board of Health at Middletown, since the third of June last; nine of which were in the city, and four at a place called the Upper

Houses, four miles north of it. Of the whole number seven proved fatal.

On inquiring of the physicians concerning the character of the fever, Drs. Miner and Tully stated that it was entirely different from the ordinary cases of fever occurring in that city, and that it was precisely similar to what they had been in the habit of seeing occasionally on board of vessels arriving from the West-Indies, and our southern ports. Dr. Gilbert, who has resided in the West-Indies, unhesitatingly pronounced the case of Mrs. Cotton, whom he attended, to be the malignant fever of the tropics.

On inquiring into the particular symptoms, Dr. Tully, who had been the physician to all the cases except five, and had also seen three of these previous to death, mentioned the following as the most prominent, and as characteristic, in a greater or less degree, of all the cases :—The patients were seized suddenly with chills, violent in degree, but of short duration; pains in the back of the head and neck, extending down the spine to the small of the back and legs; the countenance presented an appearance of great anxiety and dejection. In most of the patients the eyes, at the very onset of the attack, were of a dull, muddy, reddish colour, and towards the close they became yellow; the region of the stomach was uniformly the seat of peculiar distress; in some instances, the patients complained of a burning sensation in it; at others, of a heavy load oppressing them, and at others again, of a sense of vacuity, producing great uneasiness. This state of the stomach, Dr. Tully informed me, he had never seen in any case of typhus fever, which is the ordinary fever in that vicinity. In addition to this, the stomach was uniformly irritable, showing itself in frequent vomiting and retching, especially on taking any liquids. In several of the cases a dark coloured matter was thrown up. The skin was slightly moist, and in none of the patients hotter than natural. On applying the hand to the surface, a slight stinging heat was first felt, but afterwards it was found to be cooler than the temperature of the hand. In several of the cases the skin had the yellow tinge, and in one it was of a dark mohogany colour. The tongue varied in its appearance; in some it was natural and moist, in others covered with a slight greyish slime; in others again, a brown crust appeared upon it towards the close of the disease. In all the cases which Dr. Tully saw, the tongue was very red at the close of the disease. The thirst at the commencement was very great, and continued no longer than about twenty-four hours, after which they were indifferent about taking liquids. The pulse in the majority of instances beat from 80 to 90 in a minute—in a few it was as low as 50, and in every case entirely destitute of hardness and strength. The bowels in the early stages were generally very torpid, occasionally lax. In some cases coma and delirium were present—in others the integrity of the mental faculties was preserved to the last.—A tremor, unattended with coldness, was a prominent symptom. After the first twelve, twenty-four, or thirty-six hours, an evident remission of all the symptoms took place, so much so, that many of the patients said they were perfectly well: after this, great prostration of the sanguiferous system, together with an aggravation of all the symptoms, ensued. Muscular strength continued to the last, especially in some of the worst cases. In many cases bloody discharges occurred from the nose and the mouth; and speedily after death, putrefaction commenced, and bloody sanies issued from different parts of the body.

Such are substantially the prominent symptoms as detailed to me by Dr. Tully, and the observations of this very intelligent physician were in every respect confirmed by Dr. Miner, who visited the cases in consultation with him. As there could be no doubt concerning the nature of the disease, I proceeded next to inquire into its probable origin, and subsequent extension; and on this point I have been industrious to collect all the facts, that may shed any light upon a subject of such deep interest to the community at large. And such as I found them supported by the best testimony I could obtain, I shall frankly communicate.

1. As early as the 3d of June last, a seaman, by the name

of Orrin Fargo, was brought into Middletown sick with malignant fever, from on board the sloop Antelope, of Killingworth, a coaster between New-York and Connecticut. Dr. Tully attended him, and was informed that this was the seventh day since he was first taken sick. He died with black vomit on the 7th of June. From a statement of the captain of the Antelope, made to Dr. Tully and myself, it appears that Fargo had arrived at New-York from Savannah, in the schooner Milo, on the 25th of May. On the Saturday following, he had overworked himself on board the schooner, and in the evening was taken sick. He then took passage on board the Antelope, for Middletown, where he belonged.

2. About three days before Fargo died, Daniel H. Vail, captain of the sloop Antelope, in which Fargo was passenger, was taken sick with fever of the same character. The symptoms in this case were milder than in the former, and the patient recovered; but both Drs. Miner and Tully considered them so decided, that if even the case had occurred singly, and without any other succeeding it, they should have called it a case of the same fever which appears on board of vessels from the south. Capt. Vail told me that his sloop lay in Burling-slip for two weeks previously to his leaving New-York.

It will be observed that both these cases occurred anterior to the arrival of the brig Sea-Island.

3. On the 16th day of June, Wm. Harrington, tide-waiter, was brought into Middletown, from on board the brig Sea-Island, with malignant fever; on the following day he died, that being the 5th day after he was taken down. Dr. Tully saw him for the first time on the 4th day of the disease, and he had then the dark yellow hue of the skin; his eyes were yellow, and he vomited continually.

As the circumstances connected with the vessel on board of which Harrington was taken sick, must be deemed of much consequence in tracing the origin of this and the subsequent cases that occurred, I shall now relate all the material facts concerning her, which I obtained from a careful examination of Mr. Williams, the mate of the vessel. The brig Sea-Island left Saybrook about the 1st of December, 1819, and sailed for St. Bartholomews; from thence she returned to Wilmington, N. C. where she took in a miscellaneous cargo of lumber, corn, flour, &c. and then returned to the West-Indies, to St. Jago de Cuba. While there, two of her men died of yellow fever, and on the homeward voyage, a third fell a victim to the same disease. Nearly all the hands were also indisposed during the passage. On inquiring what was done with the clothes, &c. of those who died, he stated, that all the clothes which had been worn by the men during the sickness, together with their bedding, were immediately thrown into the sea. Some of the clothes of the second who died, but which he had not worn, were immediately washed very thoroughly, and then put up in a chest.

In the beginning of June the brig arrived at Saybrook, at the mouth of the Connecticut river. Here Harrington, the tide-waiter, went aboard of her, and on Saturday she came up to Middletown and anchored in the middle of the river, opposite the city. On Monday she unloaded and sent ashore part of her cargo, which consisted chiefly of sugar and molasses. At noon she weighed anchor and proceeded up the river. The next morning Harrington was taken sick, and remained on board until the day previous to his death. On the night before he was taken sick he indulged in drinking and smoking to excess.

While the vessel lay opposite the city, I have it in evidence that there was an unrestrained intercourse between her and the shore. Several persons went on board of her, none of whom have been taken sick, except the son of Harrington. On Sunday all the sailors came ashore, and remained for half the day. It does not appear that any of them went to Mrs. Childs' or Mrs. Cotton's, although they landed at no great distance from the place where these women lived, nor could I learn that any clothes were sent ashore to be washed. While the vessel lay off the town, two of her hands were discharged—a black man, who went immediately into the country, and a lad who took his clothes and bedding with him and went to his mother's, who lived under the same roof with Mrs. Bayley, who was afterwards taken sick.

4. Daniel Harrington went on board the brig while she lay opposite the town, and remained there during the whole of Sunday and the subsequent night. On Monday morning he left the brig, and was taken sick the next day with malignant fever. This patient recovered.

It may be proper to state that Wm. Harrington's house, in which both he and his son were sick, is situated in a distant part of the town from that opposite to which the brig lay. None of the attendants were taken sick.

5. The next case that occurred was that of Samuel Knight, a seaman, who had just returned from sea. He was taken on the 15th of June, and had previously been aboard the Sea-Island, though he did not remain there more than five minutes. He recovered.

6. Lucy Bailey was taken on the 16th inst. She lived in a house near the river, in Union-street, at some distance from Mrs. Childs' and Mrs. Cotton's. It will be recollected that in the same house lived the mother of the lad who had been discharged from the brig. His clothes were washed here, and his bedding put into the barn. Mrs. Bailey, however, told Dr. Tully she had nothing to do with the clothes. She recovered.

The next four cases were those at the Upper Houses. The occurrence of these cases at this place is so very singular, that it will be necessary to speak particularly of their locality, and of the circumstances attending them. They all took place in a cotton factory about four miles from the centre of the city, situated in a pleasant valley, and without any houses in the immediate neighbourhood. The distance between this and the river is perhaps about half a mile. Dr. Tully accompanied me to this place, and on visiting the factory we found nothing that was in the least offensive. From its situation, too, one would suppose that it must enjoy a constant and free circulation of air. It may be deemed by some of consequence to state, that the factory had been closed for some time, and that they commenced working in it only a short time before. There were at that time 7 girls and 3 men employed in it. Such were the circumstances at the factory when John Wild, one of the men, was taken sick. This was the 7th case.

7. John Wild, taken sick on the 19th of June, and died on the 25th. He had redness and wildness of the eye—yellow skin—vomited a dark matter, and on the last day brought up some blood. On inquiring of the superintendant of the factory where Wild had been previously to his attack, we found, that two days before, he had been on board of a vessel from the West Indies, called the brig Defiance of Middletown, and then lying alongside a wharf about two miles below the factory. This brig had arrived here the 15th of June, and sailed again on the 19th. While in the West-Indies, one of her men was sick with yellow fever. She went up the Oronoko, and brought home a cargo of hides, tallow, molasses, &c. The vessel is said to have been clean—between her and the shore there was a constant intercourse—numbers of people went daily aboard of her. I cannot learn that Wild was aboard of the brig Sea-Island, though at the time he was taken sick she lay opposite the factory, where she had grounded. It was while she lay here that Harrington was taken from her and carried down to Middletown.

8. Abigail Treat, a girl employed at the factory, taken sick on the 21st of June, two days after Wild was attacked, and with the same symptoms—red eyes—great distress about the præcordia, together with irritability of the stomach; brought up the same kind of matter. She had been at the factory for one month. She recovered; the fever coming to a crisis on the seventh day.

9. Rhoda Clark, another of the girls working at the factory, where she had been only three days, was taken sick on the 23d of June. She was immediately removed to her father's, about a mile and a half south of Middletown, where Dr. Tully attended her. She had the red and muddy eye—great præcordial distress—coma and decided black vomit. She died on the 27th.

10. Catherine Hubbard, another girl at the factory, where she had been one week, was taken sick at the same time with Rhoda Clark. Symptoms very similar to those of Abigail Treat. She recovered.

It does not appear that any of the three girls who were sick at the factory had been on board of any vessel. On the Sunday previous, some of them had been down to Middletown, but at this time the Sea-Island lay four and a half miles above that place.

During Wild's sickness, the superintendant informed us, that the girls occasionally went in to see him, and that they slept in an adjoining room, with only a wooden partition between them. Only one circumstance more concerning the factory is worth noticing, and this is, that a state of invariable general health has always prevailed about that place.

The three remaining cases occurred in the city of Middletown.

11. Mrs. Childs was taken sick the 22d of June, and died on the 29th. Concerning the nature of this woman's disease, it may be proper to state, that there was a difference of opinion between some of the physicians. Dr. Tracy, the attending physician, supposed that she had had epilepsy and jaundice, while Dr. Tully, who saw her on the 28th, pronounced it a case of malignant fever. A relation of a few of the prominent symptoms, will enable the reader to form an opinion for himself on the subject:—pains in the head, back and limbs—great præcordial distress—irritability of the stomach—cool skin—moist tongue—soft pulse—yellow skin—coma.

Mrs. Childs lived in a house in Ferry street, about ten rods from the water's edge, and nearly opposite lay the brig Sea-Island. It does not appear that any clothes from the brig were washed by Mrs. Childs, nor does she keep a sailor's boarding-house. I shall only add, that on the same day in which she was taken sick she had worked in her garden under a hot sun.

12. Mrs. Cotton lived in a house a few rods south of Mrs. Childs. She was taken sick on the 26th of June, and died on the 5th day of the disease. Dr. Gilbert was her physician, and

he called it a decided case of malignant fever. Mrs. Cotton attended upon Mrs. Childs during her sickness. Neither Mrs. Cotton nor Mrs. Childs had been aboard of any vessel.

13. Joseph Simmons, taken on the 26th of June, and died in five days after. This case was represented by Dr. Tully as one of the most malignant of the whole. On inquiring into the history of this case, I found that while the brig lay opposite the city, Simmons was at Colchester, about twenty miles distant; and that when he arrived at Middletown, the vessel had alrea-ready passed up the river. He lived in Ferry-street, only three or four rods N. W. of Mrs. Childs'. On the same day on which he was taken sick, he swam once or twice across the river.

Such is the detail of the circumstances connected with the several cases of fever that were reported. In looking around the city for any domestic cause which might have produced them, I found none that was adequate. Middletown is distinguished for the salubrity of its situation and the health of its inhabitants. It is built on the slope of a beautiful hill, and most of the houses are at considerable distances from each other. The freest circulation of air pervades every part of it. The town is cleanly, and even along the river and wharves there are no accumulations of filth of any kind. The ship yard in the neighbourhood of Mrs. Childs' house (about which something had been said to induce the belief that it was very filthy) was uninclosed, and had nothing in it but a small quantity of timber scattered over its surface. There was nothing about this place which even the most distempered imagination could convert into a source of infection. The same will apply also to a distillery of rum in the neighbourhood. Indeed, nothing seems capable of explaining the facts on this subject, but to admit that, with the exception of the first two cases, which occured anteriorly, the existence of malignant fever, in Middletown was owing to the presence of this vessel from the West-Indies. Otherwise it is unaccountable why, in a place where there is a total absence of all the causes contended for by those who maintain the doctrine of domestic origin, and at a season, too, which is there usually the healthiest in the whole year, so many cases of malignant fever should occur so rapidly in succession. Besides, upon any other principle, it must seem marvellous that the march of disease should proceed with the progress of this vessel up the river. She lay opposite to Middletown, and the fever followed—she lay opposite the factory, and the fever made its appearance—she proceeded to Hartford, and there we have the official report of the physicians to the Board of Health, that at least one case of malignant fever occured, which was referrible to this vessel. It is true that all the cases cannot be distinctly traced. This, however, is immaterial. The fact, that somehow or other the disease was connected with the presence of this vessel, stares us too broadly in the face to admit of a denial; and it shows, beyond all controversy, the propriety of those quarantine regulations, which by some are ridiculed, and by others are considered so unjust and oppressive. And this seems to me to be a fair conclusion, from the facts admitted by both the contending parties on the origin of Malignant Fever.

I have thus presented to the honourable the Board of Health whatever of well ascertained fact I obtained while at Middletown, and which I conceived to have the least bearing upon the elucidation of so important a subject, without regard to preconceived opinions or conflicting theories.

I must again make my acknowledgments for the uniform politeness of the mayor and physicians of Middletown. To Doctors Miner and Tully I owe a large debt of gratitude, for the liberality with which they communicated to me all the facts in their possession, on the subject of my mission. Dr. Tully accompanied me to all the places which I found it necessary to visit, and the labour of a difficult and delicate undertaking was thus rendered comparatively light.

All which is respectfully submitted.

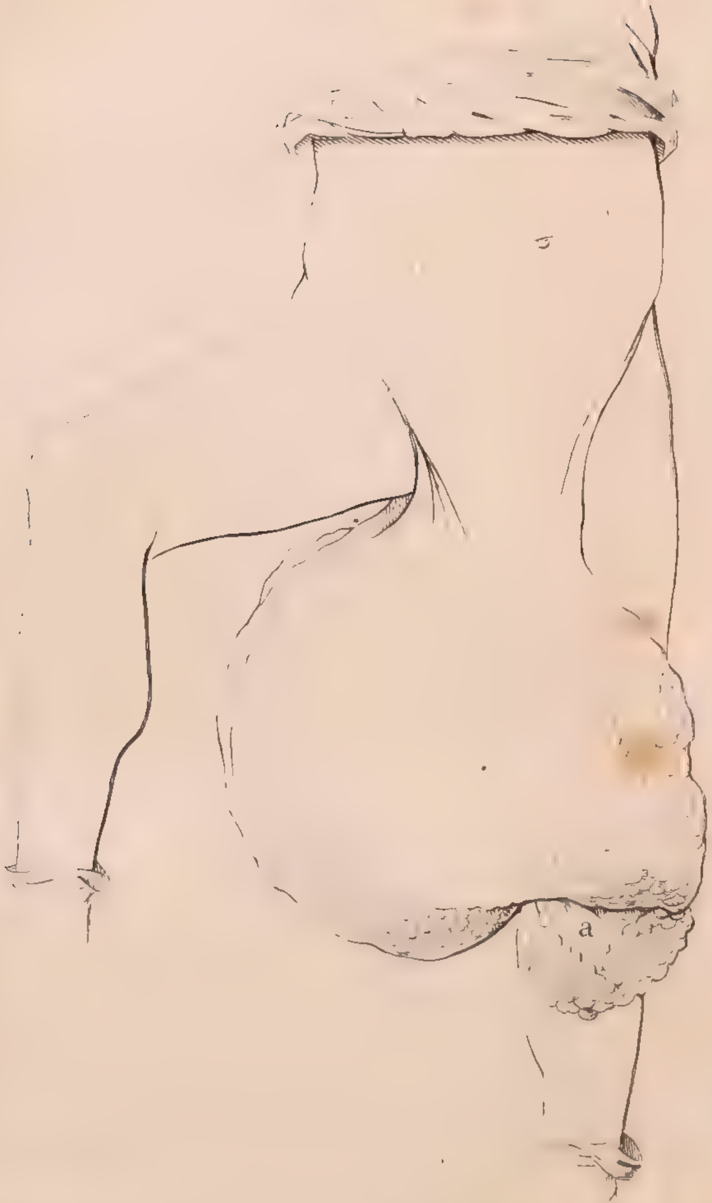
JOHN B. BECK.

ART. VI.—AN ACCOUNT *of an Operation recently performed by* PROFESSOR DELPECH, *Surgeon in Chief of the University of Montpellier, France.* Communicated by P. S. TOWNSEND, M. D., of New-York.

THE following description and engravings of a remarkable tumour, successfully operated upon by Professor Delpech, of the University of Montpellier, were presented to me by that gentleman during my late visit there. Though the reputation of M. Delpech requires no eulogium, American physicians will feel under peculiar obligations to him for having permitted the beautiful delineations, which accompany the text, to be first made public on this side the water; nothing but a notice of the operation having hitherto appeared in the European journals.

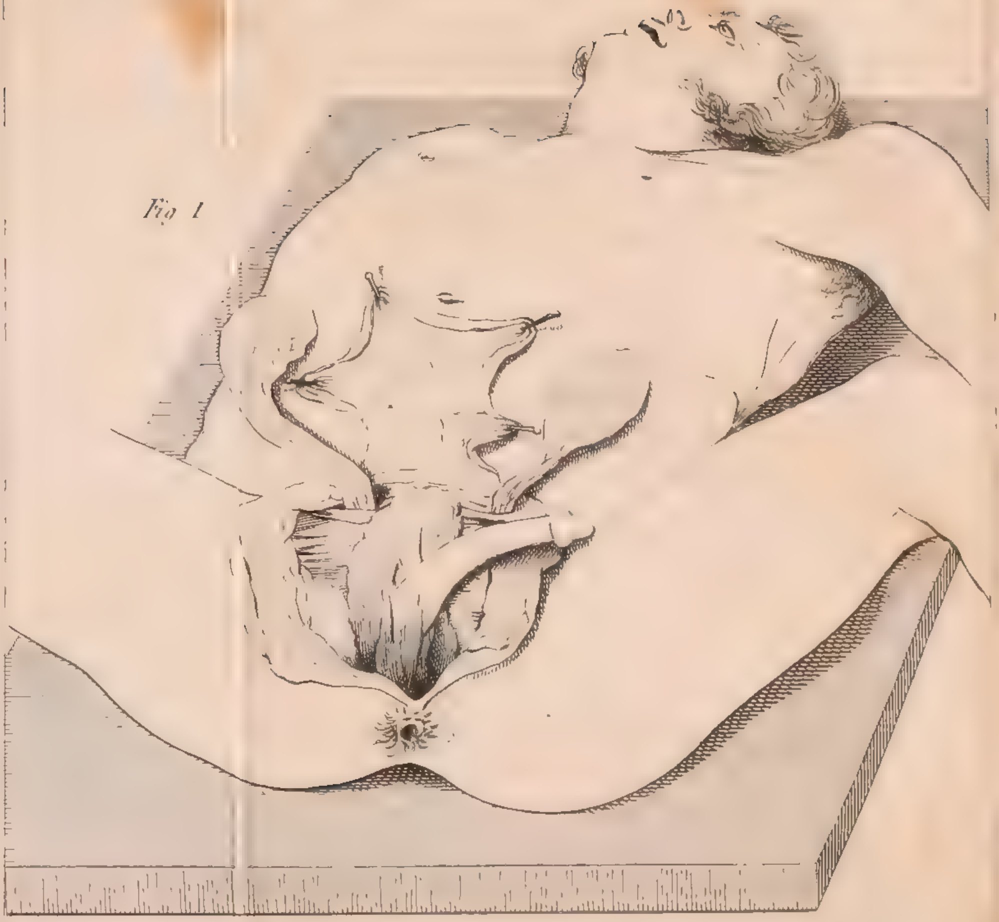
"AUTIER, a native of Perpignan, a country where symptoms of leprosy and elephantiasis are still observed, was born of a vigorous constitution, and enjoyed good health up to the age of puberty. During his intercourse with women he received several excoriations at the orifice of the prepuce, which were taken for venereal chancres. A mercurial treatment being adminstered, and this symptom not disappearing, he abandoned his profession of baker, and during the late war with Spain enrolled himself in the gendarmerie employed on the confines of the Spanish territory. The occupation of riding produced a considerable engorgement of the prepuce, which extended itself, by degrees, to all the sheath of the penis and to the scrotum. This infirmity caused him to quit the military service, and soon after even the exercise of his profession, which it appears he had resumed. Six years after, the tumour had acquired the volume which is seen in Plate I.

"The patient was admitted into the chirurgico-clinical department of Montpellier, where Professor Delpech recognised in the tumour the characters of elephantiasis; and having determined that the testicles and the penis were sound, and that



View of the size of the Tumour. — The urine which passed involuntarily, and the ejections of semen were both made through the orifice a, *which corresponded to the orifice of the prepuce.*

Fig. 1



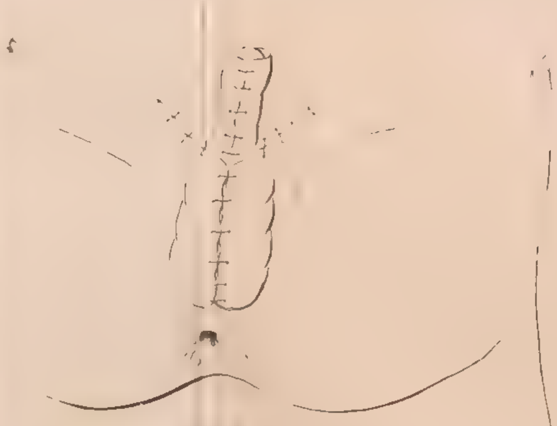


Fig. 2

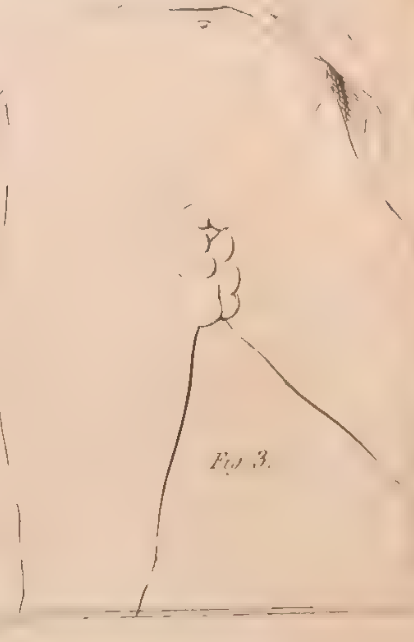


Fig. 3.

there were no other symptoms of the same affection, he conceived the following operative plan :

1. To make two semicircular flaps with the sound skin on the two sides of the neck of the tumour, each one commencing at the corresponding inguinal ring, and ending before the anus.

2. In the interval of these two flaps to form a third with the sound skin of the anterior portion of the neck of the tumour.

3. To uncover, right and left, the testicular cords ; to ascertain if there was any hernia ; to follow then the course of the cords in the tumour down to the testicles ; to isolate these last with their cords, and to place them upon the abdomen.

4. To slit open from below up, the swollen sheath as far as the glans ; then to dissect and denude the penis, and to place it on the abdomen.

5. To take away all the tumour in dissecting the perineum.

The operation is seen accomplished in plate II. figure 1. It lasted fifty minutes, and the tumour weighed sixty pounds.

The testicles were enveloped in the two lateral flaps, which went to form a new scrotum. The penis was enclosed in the middle flap, which formed a new sheath. The appearance of the sutures, and their relative position, is seen in plate II. figure 2.

In six weeks after every thing was cicatrized. The only inconveniences which followed the operation were a deep seated spasm, relieved by two doses of opium the same day of the operation, and a slight fever the five following days.

A small part of the anterior flap perished by gangrene, so that the penis has no prepuce. The scrotum is well formed, and the testicles move freely within it. This state of things is seen in plate II. figure 3."

Professor Delpech was under the necessity of digging down eight or ten inches through the immense mass of fat which formed the tumour, before he discovered any vestige either of cord or testicle. This was the occasion of considerable detention in the progress of the operation.

The casts of the tumour and of the operation in wax which the Professor showed me in his private cabinet, are extremely beautiful and accurate.

ART. VII. OBSERVATIONS *on the Medicinal Properties of the* SANGUINARIA CANADENSIS, *in a letter to* MR. FITZGERALD BIRD. By JOHN W. FRANCIS, M. D., Professor of Obstetrics in the University of the State of New-York, &c.

New-York, March 19th 1822.

DEAR SIR,

I am much pleased to find that you purpose to investigate at some length the chemical and medicinal qualities of the *Sanguinaria Canadensis*, and to arrange your observations in the form of an inaugural thesis. I am the more gratified that you have undertaken the task, because there exists great diversity of opinion as to the remedial powers of the sanguinaria. From the chemical investigations you have already instituted, you, no doubt, will be able fully to show that it possesses, as you have conjectured, a peculiar principle like that of the cinchonin.

The experience I have had of the medicinal properties of the sanguinaria, which, however, is limited, convinces me that it possesses potent virtues, and that it may be rendered a valuable article of the materia medica. I am apprehensive that it has been sometimes prescribed where neither the form nor stage of the disease warranted it, and that this circumstance has, in no inconsiderable degree, operated to its disadvantage among practitioners. Besides, its action materially varies with its pharmaceutical form as well as with the proportion of the dose. The only preparations which I have at any time employed have been the recent root pulverized, and the saturated alchoholic tincture of the recent root.

In cases of high general excitement, and in active local inflammations, given in the manner it has been sometimes recommended, it must have proved injurious, and I should prescribe it with much caution in incipient inflammation of the lungs. In this condition of the system, given in large doses, it will prove too irritating; while in moderate portions it is well known to be a tonic stimulant: in the succeeding stage, after the antiphlogistic means have been employed, the sanguinaria will often be found of service.

I have had recourse to it with essential benefit, in a long protracted and distressing affection of the chest. The patient had laboured under repeated attacks of pneumonia. and notwithstanding very active treatment, had suffered by hæmorrhage from the lungs: the consequences were much constitutional debility and habitual returns of spasmodic dyspnœa, similar to those of pertussis. The tincture of the sanguinaria, to the amount of twenty drops, three times a day, has obviated the most formidable symptoms, and given strength and vigour to the constitution.

In a case of tussis senilis which lately came under my notice, I ordered the tincture, ten drops three and four times a day, with evident relief.

You are fully aware how confidently it has been recommended by Dr. Tully and by Dr. Ives of Connecticut in the early stage of croup. The late Professor Barton, of Philadelphia, believed the sanguinaria would prove a useful medicine, particularly on the foundation of its emetic and expectorant effects, in cases of cynanche maligna, in cynanche trachealis, and other similar affections. "Its properties," adds he, "seem to be considerably allied to those of the Seneca snake root, which has been so beneficially employed in the same cases."* I have no experience of the remedy under these circumstances; and should be fearful of the results of so temporizing a practice. In the management of croup, after the active form of this disease is gone by, and when the membranous effusion exists, the sanguinaria may, perhaps, be of utility, on account of its active emetic powers. I have known ten drops of the tincture induce immediate emesis; yet I should not be disposed to place reliance on it in croup. In so hopeless a condition I am aware of nothing comparable to the free use of the vitriolic emetics.

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* Collections towards a Materia Medica, part ii. p.40.

In a formidable case of acute rheumatism occurring in a gouty habit, I lately administered the tincture of the sanguinaria with good effect, having previously prepared the patient by copious bloodletting, cathartics, and sudorifics. The quantity taken was thirty drops three times a day: it improved the general tone of the system, and gave vigour to the digestive organs.

On the authority of Schoepf and Colden the sanguinaria was long ago noticed as an antidote to the poison of venomous animals, and as a remedy in bilious and other fevers; and the late Dr. Macbride, in his letter to Dr. Bigelow,* has given it the weight of his recommendation, " in torpor of the liver, attended with colic and yellowness of the skin." In such cases where our pathology is founded on the debilitated state of the system, and tonics are indicated, the sanguinaria has probably been a serviceable article; for by its tonic powers, it must be available in these cases.

Dr. Levi Wheaton, of Providence, Rhode Island, a practitioner of great respectability and experience, states to me that the sanguinaria has became a popular remedy among the inhabitants of that place. As an emetic he himself has often prescribed it with the happiest results; sometimes to the extent of fifteen grains of the pulverized root at a dose: its action is for the most part almost immediate, and is not followed by that prostration which generalty ensues from the operation of this class of medicines.

It has been repeatedly observed, that the pulverized root of the sanguinaria, applied to ill conditioned ulcers with callous edges and an ichorous discharge, has produced an altered action in the part, and induced a healthy state. This I have several times witnessed. In a formidable ulcer of the leg of long standing, the sequela of a badly treated venereal affection, I found the pulverized root, made in the form of ointment, with simple cerate, applied a few days, answer all that could have been wished. In several trials, made in cases of obstinate ulcers occurring in the New-York Alms House and Penitentiary, under the practice of Dr. Dyckman, the surgeon of that institution, I have also been fully satisfied of its escharotic powers. Indeed, for the purpose of an escharotic it has been strongly recommended by Professor Smith.

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* Medical Botany, vol. i. p. 32.

An intelligent friend, Mr. John D. Hunter, now of this city, who has resided for about fifteen years among the western tribes of Indians, informs me that the sanguinaria is held in the highest estimation by them, and that free use is made of it in various disorders. The Chippaways, the Shawanees, the Osages, and many other tribes of Indians, who inhabit the countries watered by the Mississippi, consider it a powerful stimulating tonic, and accordingly prescribe it freely in pulmonic affections, and in acute rheumatism, after active sudorifics have been employed. Wherever there is much febrile excitement, or great local pain, the use of the puccoon is preceded by sweating and purging.

Its reputation as an emetic and as a diuretic is also high among them. As an emetic, however, it is not in their view to be ranked with the Indian physic. Sometimes a strong decoction of the blood root, with the ground ivy, is taken by the females with the intent of bringing on a premature expulsion of the contents of the gravid uterus, when pregnancy would prove an inconvenience to them about to set out on long and fatiguing journeys. Hence we can account for their often having recourse to it, as an emmenagogue, in cases where the catamenial obstruction depends upon an impaired or chlorotic habit of body In some instances of poisoning from venomous animals it is resorted to for the purpose of promoting suppuration. As an escharotic Mr. Hunter states that the application of it in a pulverized form is very common. In short, the sanguinaria is classed among their most efficient and popular remedies.

Make such use of these facts as you may see proper, and believe me to be,

With great esteem, your friend,

JOHN W. FRANCIS.

ART. VIII. OBSERVATIONS *on the Diseases incident to certain of the North American Indian Tribes*; in a letter to Professor DAVID HOSACK, from JOHN D. HUNTER.

DEAR SIR,

In compliance with your request I now, after some delay, owing to my other engagements, communicate to you a few facts which have come within my observation, and which, to a person of your acquirements, I flatter myself will at least gratify your curiosity.

On coming from the wilderness to a populous city, where every art flourishes, and every facility abounds for the cultivation and diffusion of knowledge, and particularly where a medical school exists under circumstances so happily adapted to its objects, a superficial observer would very naturally conclude that those portions of mankind who enjoy no such privileges must, in this respect, labour under the greatest disadvantages. But for these privations this uncultivated state of society is compensated by an entire freedom from many diseases to which the civilized man is subjected; and for those diseases to which the children of the forest are liable, nature has amply provided remedies, and experience has taught the skill to apply them.

The diseases of the Indians are, for the most part, simple, and may be relieved by simples. We find diarrhœa as frequently cured by a cold infusion of a bitter root, and as often by chewing astringent barks: the diseases of the stomach by alkalis and gentle laxatives, and the worst sore eyes by washing them in the juice of the yellow root largely diluted. The Indians are in general subject to few febrile diseases: those of the intermitting order are most frequent among them, particularly of the tertian form. Those inhabiting wet marshy lands, especially such as reside on the shores of rivers which inundate their banks to a considerable extent, are somewhat subject, during the vernal and autumnal months, to bilious remitting and intermitting fevers; and those who have been reduced by the enervating corruptions of the frontier settlers have, of late years, been afflicted with typhus fever.

In the treatment of fevers the Indians almost uniformly commence by giving an emetic or cathartic, followed by sweating, which with them forms one of the principal remedies in most of their complaints. During the interval between the sweating periods, when there are symptoms of considerable debility, they make free use of tonics, which consist of bitter roots, the colombo, (the Frasera Caroliniensis.) and boneset; of the names and medical properties of which I shall more fully treat in my anticipated publication.

Where violent pains accompany fevers, and are fixed in particular parts of the body, they foment with stimulating decoctions of herbs, and apply blisters on the pained parts, with hot poultices of the bark of moosewood pounded, united with several other vegetables possessing similar properties. Their fevers sometimes terminate critically by forming abscesses in various parts of the body. These terminations mostly occur in the glands of the throat or on the large joints of the extremities. These discharges are always encouraged by the application of warm emollients to the parts, it being the place, as they term it, where the disorder goes out: these local affections sometimes injure the limbs, or deform the body.

Rheumatism, which constitutes one of their most troublesome complaints, is treated by bleeding, sweating, purging, unctions, frictions, bathing, fomenting, &c. If there be local swelling in particular parts they often scarify with a knife, or sharp stone, or fracture of flint rock, but the lancet is also now used among them, and venesection at the arm. They sometimes dig a hole in the ground of a depth and capacity to contain the patient, and build a large fire of dry sticks and brush over it; upon this they pour water and get in as warm as they can endure it. Bushes and leaves are placed over it in order to retain he vapour; in this situation, almost suffocated, they often remain an hour or more. When in, they drink warm teas of sweating herbs, and on coming out wrap themselves in a blanket or robe to prevent taking cold. This is a much more disagreeable mode of sweating than the sweat oven, a description of which will be given in another place. They submit to the above mode in obedience to an opinion among them that violent diseases require violent remedies to drive them out. It is much practised among the Kickapoos and Shawanees. Their practice is tolerably successful in this disease; though from almost continued exposure to the causes which originally produced it, they are very liable to relapses; their mode of life in hunting and war, subjecting them much to the influence of wet and cold, and the uncertainty of supply rendering them more frequently victims of alternate abstinence and excess. Few of the men arrived at common age, escape this tormenting disease, and so sensible are the rheumatic to the varying temperature of the weather, that they always tell, or profess to tell, an approaching change in the state of the atmosphere; and I am disposed to think the opinion entitled to some credit, as they generally make much complaint about such times. It is well known they seldom or ever complain without great cause, and frequently endure, without betraying the smallest degree of visible displeasure, the most excruciating pains.

Pulmonary consumption is a disease, it has been said, that does not at all exist among the Indians; and some who admit it to be one of their maladies, confine it to those alone who have been unhappily debauched by the evil spirit of intoxicating liquors. The remark, however applicable to those tribes concerning whom it was stated, is far from being universally true; many facts, too well established to admit of controversy, testify the reverse; facts familiar to men, who could not be deceived in the character of a disease, the symptoms of which are so clearly marked. Nor is the disease confined to those alone who have been the dupes of that demoralizing beverage, whiskey, the article of traffick most generally taken among them by some of the traders, though it must be admitted to be much more frequent among such than among those who have never suffered by its abuse.

Those cases of consumption which I have witnessed among the Indians, and which may be ascribed to the intemperate use of ardent spirits, do not appear to me to be the direct effect of the spirit, but by rendering them incapable of taking care of themselves and exposing them to all the attendant train of evils necessarily flowing from a loss of that sense of propriety, so necessary to our comfort and safety. The Indians have in many instances contracted the views and habits of civilized society, without adopting any of its improvements. They have lost much of their native vigour and activity by this unhappy intercourse, without improving their habitations and dress: added to this they are less qualified for those more temperate and salutary pursuits, on which their health and subsistence depend. When an Indian goes into the white settlements and becomes drunken, he commonly lies out, be the weather as it may; when he rises from his stupor, he frequently takes another potation, and again places himself in the same situation. I have seen cases of this kind occur, and the practice kept up for days in succession, during which time very little food or exercise is taken. The effects on them are the same as on the whites: inflamed red eyes, with habitual weeping from them; a sallow bloated complexion, and trembling of the joints; indigestion, loss of appetite, and general debility, are the usual consequences of drunkenness among the Indians. These symptoms certainly predispose to diseases which the system otherwise might resist. And when this habit is for a considerable time continued it generally terminates the life of the subject, either by diarrhœa, dropsy, or pulmonary consumption. Independent of this cause, however, as before observed, consumptions exist among the Indians. Its approach is commonly gradual and almost imperceptible, and the symptoms are nearly as follows: a general dryness of the skin, alternate accessions and intermissions of fever, and shooting pains through the breast characterize the first state of the disease: cough and a dry state of the tongue and mouth, hurried respiration, a fixed pain in the breast, the second state, and fetid, purulent expectorations, more constant coughing, especially in the morning, or on any excessive exercise, night sweats, and diarrhœa, are common precursors of the closing scene. Some persons whom I have seen die with this disease have swelled feet and legs just before death. The females among them are less subject to consumption than the males I believe from what I have been able to learn that among the civilized this is otherwise. I have however witnessed death by consumption in a young Indian woman not more than two years since. She was of the Shawanee tribe on the Meramac river. Her case as nearly as I can recollect was as follows: she was about eighteen years of age; she went in company with a number of her tribe to Jackson, a small town in Cape Giredeau county, and state of Missouri, where from her attention to a drunken parent, she exposed herself so much as to cause an interruption in that periodical discharge so essential to the health of females. This was followed by a cough, for which marriage was recommended, to which prescription she submitted: the effect was palliative, she had a child in about a year after, and then became consumptive, and died within a few months after that event.

It would be useless to enumerate all the superstitious charms and incantations practised in the cure of this as well as other diseases among these people; some of which their white neighbours have learned them, and they owe the ignorant whites nothing in this respect; for some of them have as studiously copied their tawny brethren.

Their most efficacious remedies are emetics, sudorifics, stimulants, and tonics. When there appears to be an oppression in the breast and difficult breathing, they give an emetic. This is followed by sweating, and I believe to an injurious extent. They indulge in it till they become debilitated and feeble almost to fainting. They use internal sudorifics, also, which consists of nauseating warm teas, of roots, and herbs; and their stimulants and tonics are generally aromatic bitter roots and barks, plants, &c. They inhale the smoke of burning pitch also. The steam arising from bitter herbs placed on hot rocks, with water poured on them, is also part of the curative process. Local applications, when there are fixed pains, are also resorted to. These consist of little skins filled with hot wet ashes, and bitter herbs applied immediately to the pained parts as warm as the patient can endure them. The warm alkalized bath is sometimes resorted to.

I have thus, sir, in a very imperfect manner, briefly stated to you a few facts which have fallen within my observation at a period of my life which necessarily precluded all the advantages of education: should they afford you any gratification it will be a pleasure to me. The subject is far from being exhausted, and only requires an abler hand than mine to render it interesting, which I hope, at some future day, to see prosecuted with success.

With sentiments of the highest respect, I am your greatly obliged friend,

JOHN D. HUNTER.

ART. IX. *Observations on the* LYCOPUS VIRGINICUS, *or Bugleweed. By* J. SMYTH ROGERS, M. D. and JAMES M. PENDLETON, M. D.

To the Editors of the New-York Medical and Physical Journal.

GENTLEMEN,

The Lycopus Virginicus (bugleweed, or water hoarhound,) having attracted considerable attention as a remedy for different forms of pulmonary complaints; and its use being sanctioned by several respectable practitioners in this city and its vicinity, the subjoined extracts from notes of some cases in which it was administered, may prove not uninteresting. They are reported, not so much on account of their intrinsic importance, as from a hope, that they may afford sufficient inducements to a full investigation of the properties of a plant, which, if it deserve *one half* the credit given to it, will rank among the most valuable of our remedies in the treatment of a common, and most fatal class of diseases.

With respect, we are, Gentlemen,

Your obedient servants,

J. SMYTH ROGERS,
JAMES M. PENDLETON.

New-York, June 1st, 1822.

Case 1st, is that of a lady, of a delicate frame, phthisical predisposition, and subject to frequent hæmoptysis. At the time we saw her, she was not under the care of any physician, but having been accustomed, for several years, to watch the approach of her attacks, she became well acquainted with their premonitory symptoms: on their accession, she had for some months employed the bugle-weed, with material benefit: it was taken in the form of infusion, to the extent of a pint in 24 hours, and its effects were, to lessen excitement, to allay irritation of the chest, and thus to ward off the threatning hæmorrhage. From the result of frequent trials, she was so thoroughly convinced of its efficacy that she never left home, even for a few days, without a portion of the herb.

Case 2d. P. B. a labourer, aged 28, naturally of a strong constitution, received a violent blow on his breast, the consequence of which was a severe illness, which confined him for several weeks to his house. By the aid of a sea voyage, he was partially restored to health, but on his arrival in port, having indulged freely in the use of spiritous liquors, he was suddenly seized with hæmorrhage from the lungs, a complaint from which he had before been free: it was attended by violent pain in the injured part, distressing cough, and general excitement. By the use of the ordinary remedies, the febrile symptoms were alleviated, and the pain relieved, but the bloody expectoration continued, and the cough harrassed him incessantly, obstinately resisting every means employed for its removal. Under these circumstances, the patient was directed to take an infusion of the lycopus, (one ounce of the dried plant to one and a half pint of boiling water,) in doses of a tea cup full, six or eight times a day. Shortly after he commenced its use, an evident amendment took place in his symptoms: the cough gradually subsided, the hæmorrhage was checked, and he regained his health.

The 3*d* patient to whom this remedy was prescribed, was a coloured man, aged about 30, of an uncommonly large frame, but from laborious employments, and intemperate habits, subject to frequent alarming hæmorrhage from the lungs: within the preceding year, we had attended him in two attacks, in which he was relieved by active venesection, blisters, neutral salts, and other remedies indicated in such affections. In the present case, the symptoms were more urgent, and the hæmorrhage more profuse, than in either of those which preceded it: having acquired confidence in our medicine, from the result of previous trials, we directed him to commence with it, immediately after lessening the force of circulation by blood letting. In 48 hours the hæmorrhage ceased, and on the fourth day, our patient considered himself nearly as well as before the attack.

Case 4th. The situation of this patient was such as to preclude all hope of permanent benefit from any course of remedies: she was a woman far advanced in life, and when visited, was lying in the last stage of pulmonary consumption: all that remained for us to do, was, to render as comfortable as possible, the last moments of her existence. She was extremely debilitated; but the only symptom producing much uneasiness was a cough, which both by day and night disturbed her rest, and exhausted her little remaining strength. A combination of tonic and anodyne medicines was at first found of partial utility; in a few days, however, they excited so much disgust, that she could not be prevailed upon to continue their use. We then directed the infusion, in small and respeated doses: this proved a grateful drink, and she took it freely. Within three days the cough entirely left her, and expectoration was effected without the slightest efforts: she remained exempt from the cause of suffering during the remainder of her illness, which, however, terminated fatally in a few weeks.

Our 5*th case*, was that of a middle aged woman, of a feeble constitution; from the form of chest predisposed to consumption; and liable to hæmoptysis with almost every cold. She was likewise subject to hysteria, and her general health was bad. In the last return of hæmorrhage, in which we prescribed for her, as the symptoms of excitement were very slight, we commenced the administration of the infusion, in the usual doses, without any preceding remedies: she considers herself much benefited by it, and was, very shortly, convalescent

The 6th and last case of which we have any minutes, was that of a scrofulous boy, whose situation from our first visit, as regarded prospect of cure, was desperate. He was, at times, much tormented by a cough; a variey of medicines had been given to palliate his complaints without much benefit; recourse was at last had to the lycopus; it was found of more service in relieving the cough than any thing he had previously taken, though that symptom continued more or less troublesome until his death, which took place more than nine months after confinement to his chamber.

In reviewing the preceding reports, we would remark, that the only case in which any circumstance rendered the effect of the bugleweed at all equivocal, was the second: in this an anodyne was given at night, and on one or two occasions in the day, while employing the infusion: this diffiulty is in a great measure obviated by the fact, that anodynes had been previously prescribed with the other remedies, with but little advantage; and that the improvement of our patient, after he had resorted to the new medicine, was more marked and rapid than before its use. This patient noticed a peculiarity, attending the operation of the infusion, which appears in some way connected with its curative properties: the draught uniformly caused a sense of codness, which he compared to the sensation produced by "cold water running down into the stomach:" that this was independent of its actual temperature is evident from the fact that other drinks, having no greater degree of warmth, did not cause a similar feeling. The same remark was made by the lady whose case is first detailed: in her, the coldness amounted to a temporary, but universal chilliness, followed by a diminution of the febrile heat, which accompanied her complaints.

Before concluding our paper, it will be proper to mention a fact, which should be known to those who are disposed to employ the remedy in the cases to which it is adapted. There are two species of the lycopus, found in this part of the country, which closely approach each other in their botanical characters, but differ materially in their sensible qualities, and in their medicinal properties: it is unnecessary here to give a minute description of the two plants; they may readily be distinguished by attention to the following particulars:—The flowers, which are very small, grow close by the stem in both species. In the L. Europæus (the *spurious* plant) the calyx is sharp and thorny; in the *genuine*, the L. Virginicus, it is soft. The taste of the *spurious* is purely and intensely bitter: that of the *genuine* much less so; and with its bitterness there is a peculiar flavour, which is wanting in the other. When a quantity of the *genuine* plant has been kept in a close drawer for a few hours, it exhales an odour somewhat like that of lamp oil; the smell of the other is merely herbaceous. These *popular* marks of distinction are the more important, as the dried plant only is offered for sale, in which state its botanical characters are not so easily discovered. By far the larger proportion of the herb, which is found in our shops, is of the *spurious* kind; or *a mixture of the genuine with a large proportion of the spurious:* to this may probably be ascribed the disappointment many have experienced in the use of the lycopus, as the medicinal properties of the two plants differ widely; and many cases to which the L. Virginicus might be suited, would be materially injured by the L. Europæus: the latter plant was formerly in extensive use in France, as a substitute for cinchona, and considered capable of fulfilling every indication for which the Peruvian bark is usually prescribed. We were informed by the person who supplied most of our shops with the bugleweed, that he was not aware of any difference in the effects of the two plants, although he could readily point out their distinctive marks; but that he had been in the habit of gathering indiscriminately the L. Europæus and the L. Virginicus, the former of which is much more abundant than the latter.

We regret that the impossibility of procuring a sufficient supply of the genuine plant obliged us to discontinue our investigations: we hope, however, to resume them, so soon as we are in possession of the means of doing so with advantage. We trust the preceding details may induce others to engage in a similar examination, and that the virtues of the plant may be fairly and fully tested.

ART. X.—A CASE of PHLEGMASIA DOLENS, *occurring in an aged female.* By JOHN B. BECK, M. D.

THE following case, although not a solitary one of the kind, is nevertheless of very rare occurrence; and as it bears upon some popular theories concerning an interesting disease, I have judged it of sufficient importance to place it on record.

In the month of May last, I was called to visit from the Dispensary a female patient, whom I found with her left leg uniformly enlarged from the toes to the groin, about one third beyond the natural size of the limb. Most of the symptoms characteristic of phlegmasia dolens were present. The limb was tense, shining, elastic, and exceedingly painful. No œdema was discoverable in any part of it. The glands in the groin were not found to be enlarged in any stage of the disease, after I was called in, nor could I learn that they had been so previously. Generally the leg was extremely hot, although occasionally the patient complained of a sense of coldness in it. The left labium had remained unaffected by the swelling in the neighbourhood. I saw the patient first on the fourth day after the disease had made its appearance, and she informed me that it commenced with a feeling of deadness in the toes, heel, and upper part of the foot, which was shortly succeeded by severe pains in the part, after which the foot began to swell. By the succeeding day the swelling had ascended to the knee, from which it gradually proceeded to the groin.

On the fifth day from the commencement of the disease, the swelling began to decrease, and on the seventh the limb was reduced to its natural dimensions, with the exception of the upper part of the thigh, the enlargement of which did not completely subside until the tenth day.

The subject of the preceding case is a native of Ireland, aged fifty-two years, and of a spare habit of body. Her menses, which had previously been regular, ceased about six years ago. She is the mother of six children, the youngest of whom is seventeen years of age. Her labours have in general been natural and easy, and none of them have been succeeded by any unpleasant consequences. She has never been troubled with fluor albus, and has enjoyed an uninterrupted series of good health, until within the last two years, during which she has been occasionally afflicted with rheumatic pains in her arms and legs.

In the treatment of this case the symptoms yielded rapidly under the use of active cathartics, with tepid applications to the part, succeeded by stimulating liniments, frictions, &c.

I cannot dismiss this case without adding a few reflections.

1. It has been common with almost all the popular writers on Phlegmasia Dolens, to consider it as inseparably connected with the puerperal state, and all their theoretical doctrines concerning its proximate cause are, accordingly, built upon this assumption. This error, for such it must be admitted to be, I am surprised to find countenanced by late writers on this subject, and especially by a very recent British author, in a work* eminently characterized by sound sense and practical wisdom.

Dr. Ferriar, I believe, was the first person who advanced the opinion, that so far from its being dependent upon the puerperal condition, it might actually occur in males, and he relates a case to this effect which fell under his own care.† Subsequent observation has confirmed this fact beyond the possibility of dispute. The recorded instances in which this affection has occurred in males, though rare, are clear and decisive, and the case which I have detailed is no less conclusive on this point. I conceive it then to be established, that this disease may, and not unfrequently does arise from causes unconnected with parturition.

2. This disease is usually described as invading only the lower extremities. In one of the latest and best works on Nosolo-

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* Observations on the use and abuse of Mercurial Medicines in various diseases. By James Hamilton, Jun., M. D.

† Medical Histories and Reflections. p, 341. Am. Ed.

gy, it has hence been denominated *Cruritis.*[*] There are some facts, however, which seem to favour the belief that it may occasionally appear in the upper extremities. Dr. Ferriar first remarked, that this might be the case, and he even goes so far as to assert, that it may invade any part of the body. He says, "I see no reason for supposing, that this affection is necessarily confined to the extremities, particularly to the lower limbs. The causes which produce inflammation of the lymphatics in one part of the body, may occasion it in the same order of vessels, when applied to them in any part of the body."[†] In addition to this he describes the case of a gentleman attacked with this disease in his left leg and thigh, who previously had a painful enlargement of the left arm, "but not" he adds, "to a degree equal to the disorder which I have described."[‡] The account is certainly not very precise, but it is fairly to be inferred, that the affection of the arm was similar to that of the leg. Without resting, however, at all upon this doubtful evidence, the testimony of Dr Lowder is explicit on this subject. He says, "I have seen this disease affecting the upper extremities."[||]

The following case, communicated to me by Dr. Heermans, goes so directly to confirm the foregoing facts and opinions, and is of so interesting a nature, as to render unnecessary any apology for introducing it in this place.

"The subject of the following disease was a young man of nineteen years of age, of a nervous temperament, and had been afflicted, from a boy, with a painful affection of the muscles of the leg, unaccompanied, however, with any swelling or tumefaction of the part. This was found to be particularly troublesome after violent exercise or exposure to damp and cold weather. In November, 1814, he took a long journey on horseback, during the two last days of which he was exposed to constant rains. Three or four days after his return home, he complained of a pain in the ham of the leg, with some inability to move it. The pain soon increased, and swelling commenced and extended rapidly upwards to the groin. The sensation was so acute, that he could not bear the least thing to come in contact with it. In about thirty hours, the limb was enormously swelled, had much the appearance of an anasarcous leg, though differing from it, in not being so uniformly smooth and evenly distended, nor did it pit upon pressure from the fingers.

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[*] A System of Practical Nosology. By David Hosack, M. D. F. R. S.

[†] Histories and Reflections, p. 345.

[‡] Ibid. p. 342.

[||] Ibid, p. 362.

The swelling soon commenced in the other groin, descending as rapidly as the other had ascended, with the same sensation and appearances. The swelling now continued in both legs down to the extremities of the toes. On the fourth or fifth day he complained of the shoulder, and some swelling was perceived about the joint. This soon increased, but not with the rapidity and violence, that it manifested in the lower extremities. It gradually extended down the arm of one side, and was soon followed by the other. On the ninth or tenth day from the first attack, the swelling and pain had generally subsided, except the left arm, which remained enlarged for six or eight weeks before it subsided. The pulse at the commencement of the disease was frequent and chorded; the stomach was affected with slight nausea; the bowels costive, but easily moved by cachartics.

Viewing the disease as of an inflammatory character, I had recourse to bleeding, cathartics, and diaphoretics. Fomentations were freely used, together with friction frequent and active, after the general inflammatory symptoms had subsided. Bark and wine with elixir vitriol were given during his convalescence."

There can be no question, I think, concerning the nature of the disease described by Dr. Heermans, and if so, it serves to expose, in a still more striking manner, the inadequacy of almost all the theories which at various periods have been advanced on this subject.

ART. XI. *Account of the* SALMO OTSEGO, *or the* OTSEGO BASSE, *in a letter to* DR. JOHN W. FRANCIS, *from* DE WITT CLINTON, L L. D., Governor of the State of New-York.

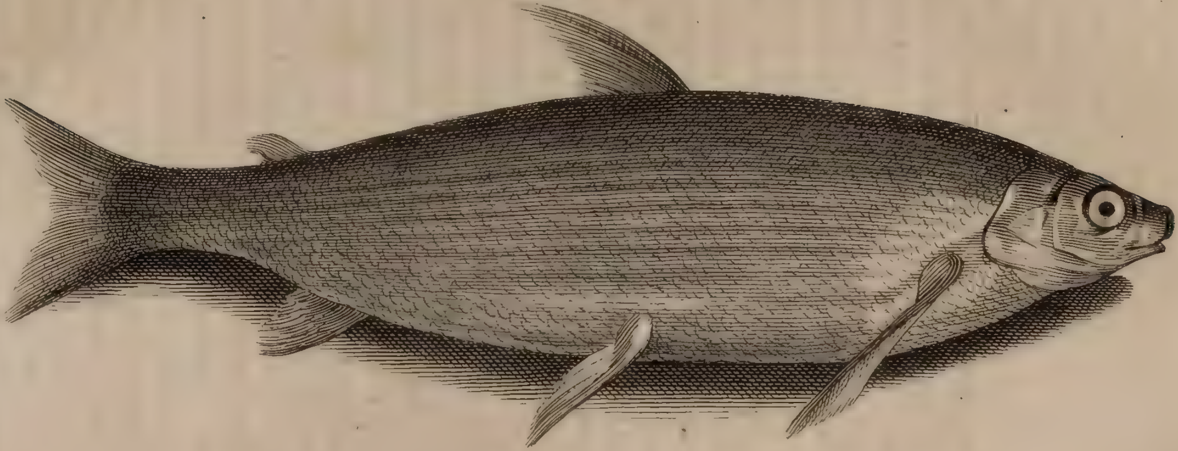
Albany, May 16, 1822.

SIR,

I send you, for your excellent work, an accurate and beautiful delineation of a fish, caught in the Otsego lake, the principal source of the Great Eastern Branch of the Susquehannah river. This painting was executed by Ezra Ames, Esq. an eminent artist of this city; and the representation of this fish, which is called Otsego Basse, is as large as life. It was seventeen inches in length, five in depth, and one in thickness; and it weighed two pounds six ounces.

The pupil of the eye is black; the iris silvery; the opercula spotted with yellow on a silver ground; the mouth small, and exactly like that of a shad, except a little protuberance of the upper lip, which is also bifid. No teeth in the jaws, palate, roof of the mouth, tongue, or throat. The lateral line straight, and hardly discernible: appearance of lateral stripes, like the Perca Mitchilli, or Roch fish—colour silvery white below the linea lateralis—somewhat darkish above it; scales small; back highly curved. The first dorsal fin has nine softish rays, three of which are imperfect: it is about mid-way in the back, and over the ventral fin. The second dorsal fin is small and filamentous, and directly over the anal fin. The caudal fin is forked—the belly not serrated.

This fish is of the abdominal order, and cannot be satisfactorily arranged under any genus of the Linnæan system. As it is, however, included in Bloch's description of the salmon, which he represents as an abdominal fish, with an adipose fin on the back, and a body covered with scales, I shall, in order to avoid the multiplication of genera. consider it as a Salmo; and, as it is probably a non-descript, and peculiar to the Otsego lake, its specific name may, with propriety, be derived from the water which it inhabits: I shall, therefore, call it *Salmo Otsego*.



This fish is nearly equal to any fish that swims for exquisite and delicious food. It is among fishes what the grouse, or canvass-back duck, is among birds. The flesh is fine, white, and delicate.

It sometimes weighs six pounds: It is never found in the neighbouring lakes or streams, or in the Susquehannah river. It is not anadromus, and appears to be always stationary in the lake. It appears in shoals every spring and fall, and at other times retires to the deeps of the lake, like the black basse and perch of lake George, which are only seen periodically. The latter lake has no outlet which can be ascended by fish : This proves, conclusively, that their retreat is only into its deep waters ; and as these lakes are very deep, they afford ample space for seclusion. Perhaps the notion of Pennant that the herring proceed from the polar seas is incorrect. I cannot believe that they extend their migrations beyond the seas in which they periodically appear.

The Otsego basse is very rarely taken by the hook. It was formerly caught in great numbers by the seine—sometimes five thousand at a draught. There is now a comparative scarcity. The diminution of fish, greatly in demand, may be attributed, generally, to the havoc created by the increasing population of the country, and to the destruction of the food of fishes produced by cultivation ; but in this case it is ascribed to peculiar causes.

The spring herring, or clupea vernalis, formerly visited this lake in immense multitudes for the purpose of spawning; now their ascent is arrested by the erection of dams. The salmon trout, and other predaceous fishes of the lake, which formerly derived their subsistence from the spawn and young of the herring, being shut out from this source of supply, have turned their attention to the Otsego basse, and have thinned its numbers. The herring formerly reached this lake in the spring, and their young did not leave it until the ensuing autumn. The pickerel, which is the tyrant of the lake, and which drives away and conquers its neighbour and congener, the white pike, of the Susquehannah river, is considered the principal cause of the diminished numbers of the Otsego basse. But captivity appears to have the same effect on them as on the wolf. Near the place where the fishermen draw the seines there is a capacious fountain, in which they deposit their fishes and keep them alive. In one of their draughts they took a large pickerel, and put him in the depot. The basse held no communion with him ; and they kept apart as far as possible, without experiencing any annoyance from their formidable enemy.

The Otsego basse feeds upon worms, which appear to be abundant, attaching themselves to a concreted ropy substance found mostly in and on the grass at the bottom. It also eats those insects which appear on most waters in the spring of the year ; and it probably derives its nutriment in part from aquatic vegetables, fat earth, animal and vegetable mucus, and the power which all fishes possess, of decomposing water and converting it into nourishment.

It spawns in water not under twenty-five feet deep, and the season for this purpose is supposed to be exclusively the autumn ; but one having been found this spring in roe, and the periodical appearance of this fish being vernal, as well as autumnal, I should suppose that it spawned in both seasons ; for whenever gregarious fishes change their places of habitation at prescribed times, from the depths of the lakes or the ocean, into the shallow waters of the former, or the rivers which run into the latter, it may be considered as the season of production, marked out by nature.

I am, dear sir,
with great respect,
Your most obedient servant,
D. W. C.

John W. Francis, M. D.

ART. XII. *Description and Analysis of a new* ORE OF ZINC, by JOHN TORREY, M. D. of New-York.

[Read before the N. Y. Lyceum of Natural History, April, 1822.]

THIS mineral was discovered about two years ago, in the town of Ancram, in the state of New-York ; a place remarkable for its iron works and lead mine. It was found on taking down one of the old walls of the furnace, erected in the year 1744. It attracted notice from its great weight ; and although it resembled some common stone, it was supposed to be an ore, though of what metal was not conjectured. Several specimens having been sent to me for examination, I submitted them to analysis ; and the result was altogether different from what the appearance of the mineral indicated. It proved to be a nearly pure oxyd of zinc, and an ore of that metal distinct from any hitherto described. This induced me to make further inquiries respecting the mineral, as it would be exceedingly valuable if found in sufficient quantity ; but I have, as yet, been able to procure very little satisfactory information respecting it. Mr. Seth Hunt, who visited Ancram in October last, was informed by Mr. Thomas Ackers, manager of the lead mine, and Robert Patterson, Esq. of the iron works, that the ore was found *built into* the wall of the furnace, and that notwithstanding the repeated personal search of the latter gentleman, through the surrounding country, for the distance of several miles from the furnace, and making diligent inquiries of the ancient inhabitants, he had not been able to obtain any information that would direct him to the spot from whence it was taken. He has, however, every reason to believe that the ore was obtained within a few miles of the furnace, and, most probably, within a very short distance ; but time, through whose lapse it was lost, may again bring it to light, and furnish to the fortunate discoverer, or proprietor, or to both, the means of wealth and independence. Since I received this information from Mr. Hunt, I have heard of the discovery of a considerable quantity of the mineral in the foundation of an old house near Ancram.

DESCRIPTION OF THE MINERAL.

External Characters. All the specimens I have seen were tabular masses, possessing sometimes a distinct slaty structure ; though they generally were composed of layers without a tendency to separate, giving the mineral a striped aspect, when viewed in a direction perpendicular to the direction of the strata.

It is granular and compact.

Its colour is grayish green, or olive green.

It is perfectly opake and the surface dull.

It is harder than calcareous spar, and is easily reduced to a powder, which is of a lighter green than the mass.

Specific gravity, 4.924.

Chemical characters. Before the blowpipe it is infusible, but when the heat is intensely urged, the oxyd of zinc is volatalized, communicating a white colour to the flame. The powdered mineral dissolves instantaneously, and almost entirely without effervescence, in the stronger acids.

ANALYSIS.

The following preliminary experiments were made to ascertain the nature of the mineral.

A. Some of it in fine powder was treated with nitric acid. It readily dissolved, leaving only a small quantity of a light black residue. The solution, on being filtered, was transparent and colourless, and yielded quadrangular prisms on evaporation. These had a caustic metallic taste, and were very soluble in water and alcohol. When the nitric solution was repeatedly boiled to dryness, and a small quantity of acid added at each operation, some pure oxyd of iron was separated. To the filtered liquor the following tests were applied :

(*a.*) The caustic alkalies produced a white precipitate, which was entirely re-dissolved by an excess of aklali.

(*b.*) Prussiate of potash occasioned a white precipitate.

(*c.*) Hydro-sulphuret of potash produced the same effect.

(*d.*) Copper, iron, and other metallic rods immersed in the solution did not occasion any precipitation of metal.

(*e.*) Muriatic acid did not disturb its transparency.

(*f.*) Oxalic acid threw down a white precipitate.

B. There remaining no doubt that the principal metal of the ore was zinc, the black insoluble residue was next examined. It was not acted upon by acids, except strong nitric and sulphuric acids, which it appeared to decompose. When made into a ball with a little mucilage, and ignited, it burned almost entirely away without flame or vapour. Projected into melted nitre, it caused a violent deflagration. It was nearly pure *carbon.*

C. One hundred grains of the ore, in fine powder, dissolved immediately in diluted sulphuric acid ; leaving one grain of the black powder, which was ascertained in the preceding experiment to be carbon.

(*a.*) The solution was clear and colourless. It afforded flat four-sided prisms on evaporation, having all the properties of sulphate of zinc. These were re-dissolved in water, and carbonate of potash added until precipitation ceased, and the liquor was boiled to ensure the complete decomposition of the metallic salt. The carbonates of zinc and iron were thus obtained.

(*b.*) To separate the latter, the whole was re-dissolved in acetic acid, and afterwards boiled to dryness. The acetate of iron was decomposed, leaving the base in a state of per oxyd weighing 3.50 grains.

(*c.*) It now remained to determine the quantity of oxyd of zinc, which was done as follows : The solution obtained in the preceding experiment, after separating the iron, was precipitated by carbonate of potash. The carbonate of zinc thrown down was tested with ammonia, to be certain of the absence of manganese ; but this alkali dissolved the whole of the precipitate, and proved it to consist of zinc alone. On evaporating the ammonia, and calcining the carbonate, 93.50 grains of oxyd of zinc were obtained.

The result of the analysis is :

Oxyd of zinc,	93.50
Oxyd of iron,	3.50
Carbon,	1.00
	98.00
Loss,	2.00
	100.00

The red oxyd of zinc of New-Jersey, discovered by Bruce, resembles our mineral in composition more than any other ore yet described. It however differs essentially in containing 12 per cent. of manganese, and in the absence of iron. Its external characters are altogether dissimilar. To distinguish it from Bruce's mineral, this may be called *green oxyd of zinc* ; though I am aware that the name is objectionable, as there is ut *one* oxyd of zinc, which is *white*.

It is much to be regretted that a mineral so interesting and valuable should not be better known. We hope the active search which is making for it will not be without success. It is by far the richest ore of zinc known ; containing, according to Thompson, 75.31 per cent. of metal. The red zinc ore of New-Jersey, beside the manganese which it contains, is always intimately mixed with more than half its weight of Franklinite, from which it is impossible to separate it by mechanical means ; while the ore of Ancram is so pure that it can be used without any preparation, either for extracting the metal, or for the manufacture of brass.

Art. XIII. Observations *and* Geological Remarks *on the* Minerals *of Paterson and the valley of Sparta, in New-Jersey.* By Thomas Nuttall, F. L. S. London.

A cursory geological sketch, sufficient to excite a closer attention to the subject in those who follow the same route, may not perhaps be a superfluous introduction to the more immediate subject of the present communication. In my way, by the usual road, to Paterson, along the banks of the Passaic, little presented itself to the observation of the geologist, except the development of the Red sand-stone formation, commencing at Bergen heights, four miles west of the city of New-York. In this ridge, which so immediately succeeds that of Hoboken and its *transition* serpentine, (for so I now consider it,) existed the famous copper mine of Mr. Schuyler, now abandoned, and which only forms, in fact, a small portion of a metalliferous bed, extending, at least, as far as the banks of the Rariton.

On approaching the town of Paterson, a scenery more diversified and romantic presents itself, and the surrounding cliffs and precipices, as well as that which produces the picturesque cataract of this place, consist of beds of trap, well characterized, and reposing on the Red sand-stone. This formation, which has been so carefully examined by my friend Judge Kinsey, of this place, is peculiarly interesting to the naturalist. The trap contains, as usual, in its dispersed cavities, nodules of prehnite, of mezotype, stilbite, agates, and, in one locality, fine crystals of datholite or siliceous borate of lime. Sometimes it passes into an amygdaloid, as in the Derbyshire toad-stone, from which it can scarcely be distinguished, except by the rarer substances contained. In some of the amygdaloidal cavities lined with crystallized carbonate of lime, occasionally also occur small greenish crystals of the datholite; others are exclusively lined with druses of crystallized or lamellar chlorite, which indeed enters largely into the composition of the wacke or softer trap of this region. In Derbyshire this formation is overlaid by what is called the mountain limestone, one of the more ancient beds of the secondary formation; here the trap and amygdaloid form the utmost summits of the hills, which in height approach to mountains. Its junction with the *red* sandstone is marked with some peculiar characters, highly imposing to those who feel inclined to favour the Plutonic theory. I must confess that at the point of junction of these formations the trap is peculiarly ambiguous: it presents a regular porous structure; not, however, any thing like *pummice;* and many of the pores, if not *all*, seem dependent on the decomposable nature of the carbonate of lime, which still in the interior occupies those supposed pores. There are, moreover, in some few places, larger cavities, appearing somewhat glazed; and rugose masses, exceedingly like slags; these are, however, appearances which ought to be weighed with caution, and are insufficient for demonstration. The porous quartz of Providence township, in Pennsylvania, and of West-Chester, in Delaware, connected with the greenstone formation, is far more imposing as a volcanic production; yet distinctly traceable into a reticularly veined and compact jasper abundantly in place near to the Black Horse Tavern. This porous texture, perfectly original, is such as to render considerable masses of the mineral buoyant on water. But what are we to think of the serpentine, equally porous, in the same locality, and several others, (as near Dixon's farm, Wilmington,) which might be adduced, also gradually and distinctly passing into perfect compactness? If density alone be considered conclusive of aqueous origin, we must introduce a double system of formation into every class of rocks the globe exhibits.

In the ridge, about two miles west of Paterson, occur very perfect basaltic columns of small magnitude, (about a foot or eighteen inches in length, and from one to four inches in diameter,) presenting from four to six sides; blackish green and crystalline in the interior. This ridge throughout presents beds of columnar concretions as far as the banks of the Little Falls of the Passaic.

This basaltic formation forms the summit of every ridge, for about 15 miles northwest, to Pompton, where it ceases, and is succeeded by grauwacke, gray, white and liver brown, containing occasionally pebbles of red and brownish jasper. The affinity of this grauwacke with the red sand-stone of Paterson is sufficiently obvious, not only from hand specimens, but from identity of relative situation, for they both underlay the trap. And what, indeed, shall we think of the antiquity of organized bodies, when it is known, which is a fact, that casts of ammonites* have been found in the larger pebbles, contiguous with the surface of the red sand-stone formation, or near to the point where it *enters into* junction with the porous trap. In the lower portions of this formation, I believe, as yet, no organic remains have been discovered; and it is not unfrequently, when approaching to a slaty structure and argillaceous consistence, eminently metalliferous, as at Schuyler's copper mine, and that now working at Somerville, which presents masses of the ruby oxyd, native copper, and minute portions of native silver.

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* Collected by Judge Kinsey.

Contiguous to the western declivity of the Pompton mountains, Judge Kinsey and Doctor Mead found pale green sahlite in abundant masses, connected with a beautiful smallish grained dazzling white carbonite of lime or marble, again likewise contiguous to a formation of diaphanous greenish yellow perfect serpentine, traversed, like that of Newburyport, with silky seams of amianthus.* Connected apparently with the greenstone formation, is a large rolled or rounded mass of Labrador felspar, found by Judge Kinsey in the vicinity of the hills of Pompton.

After passing the Pompton mountains, a succession of the ridges of the Highlands present themselves, thickly strewed with rounded *debris*, so as to render even the valley lands difficult to cultivate. Passing the Warwick ridge, or Hamburgh mountains, we enter upon the valley of Sparta, where a beautiful white crystalline limestone and marble appear, occupying the valley, and rising westwardly into a low subsidiary ridge, about eight or nine miles in length, proceeding almost due north and south.

As the metalliferous deposites form here the most remarkable feature of the formation, we shall preface our examination of the minerals of Sparta, by a few remarks upon them. The first, or eastern bed, which at Franklin appears like a black mountain mass, at least 30 or 40 feet wide, consists of an ore of iron in common, scarcely at all magnetic, and with great propriety considered by Berthier as a new metalliferous combination entitled Franklinite, contains 66 per cent. of iron, 16 of zinc, and 17 of the red oxyd of manganese. On the supposed riches of this immense bed, the great furnace of Franklin was built; it was soon, however, discovered not only to be irreducible to metallic iron, but to obstruct the fusion of other better characterized ores in a notable degree. If employed in a quantity exceeding one tenth of the magnetic oxyd of iron with which it was economically mixed, the result was what the miners emphatically termed a *Salamander*, an alloy of iron, with the manganese, which resisted liquifaction, and crystallized even under the blast, so that all the metal was lost, the hearth demolished, and ten or twelve yoke of oxen necessary to drag away the paradoxical and useless metal produced.

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* The same mineral, with its usual associate, has been received from Philipstown, on the Hudson, by Professor Renwick, of Columbia College.

This deceptive mineral, now abandoned for want of skill to reduce it, occasionally presents cavities lined with regular octahedral crystals, the sides of whose pyramids to each other as well as the the angles of the common base measure by the reflecting goniometer, according to Doctor Torrey, angles of 108.15. At Franklin it is but sparingly intermingled with the red oxyd of zinc. In about two miles to the north, the Franklinite bed ceases to be any longer discoverable at the surface, but continues more or less distinct for five miles further to the southeast, or seven miles in its whole range. Three miles from Franklin furnace, at Stirling, another mountain mass or huge cliff of this formation presents itself; but here the red oxyd of zinc forms, as it were, a paste in which the crystals of Franklinite are thickly imbedded—in fact, a *metalliferous porphyry!* On the sides of the seams, abundance of octahedral crystals of Franklinite are often well developed, while those of the interior are commonly pseudomorphous. Numerous illinitions of the carbonate of zinc appear throughout the interior of the mass. This ore, merely pounded and mixed with copper, has been profitably employed, during the scarcity of the late war, for forming brass.

Often, within a few feet to the west of the Franklinite bed, appear others of well characterized magnetic oxyd of iron, but always accompanied by hornblende rock. One of the most profitable beds of iron ore was discovered on the very spot where stands the furnace; this highly magnetic oxyd so much esteemed is very intimately blended with plumbago, so as at first glance almost to resemble the siderographite of Doctor Torrey. In the latter, however, the iron is perfectly metallic; in this a protoxyd.

Among the more curious accompaniments of this metalliferous formation is the brownish yellow garnet, analyzed by Vauquelin, who found it entirely soluble in muriatic acid, which not only takes up the silex contained, but also the manganese, in consequence of its existing in the mineral at the minimum, of oxydation. The same substance has now also recently been analyzed by Mr. Seybert. It here forms a bed or vein six or more feet wide, and when occasionally in contact with the carbonate of lime exhibits imperfect dodecahedrons of a lustre and colour almost similar to idocrase.

Contiguous to the great bed of Franklinite, at Franklin, is interposed, as it were, in the crystalline carbonate of lime, which prevails to the east, a ledge of imperfect sienite, scarcely presenting any thing more than gray rocks of a binary combination of quartz and felspar. In these rocks occur beautiful opaque blackish-brown masses of garnet, of a high resinous lustre, and crystallized on the surface; it agrees in every respect, but the unimportant one of direct colour, with the melanite of Frascati, near Vesuvius; it is accompanied by a laminated epidote almost similar in appearance with the loboïte, or idocrase magnesifere of Berzelius.

Contiguous to the same locality there is a vein or two, forming as it were the junction of the sienite and carbonate of lime, consisting almost exclusively of a nearly white and compact massive or minutely lamellar augite, in some parts intimately blended with specks of violet, granular felspar, resembling petrosilex; also silico-calcareous oxyd of titanium, brown garnet, dark green granular augite, like the coccolite of lake Champlain; something of nearly the same colour, which we suppose may prove the Gahnite, occasionally in octahedrons, and minute pale bluish green prismatic and translucent crystals, which are probably phosphate of lime. These veins, which stand up in unaltered crests from the more decomposible carbonate of lime in which they are imbedded, present also on the surface druses of augite crystals.

A little distance from this vein there is another, apparently much broader in its dimensions, being a mixture in the mass, more or less intimate, so as to be often minutely granular, of green felspar of a colour from pale olive to that of carbonate of copper, black hornblende, whitish augite, mica, and occasionally silico-calcareous oxyd of titanium. In the cavities of this vein, and often considerably immersed below the surface, are, in considerable abundance, bluish green octahedrons of the spinelle, sometimes presenting truncations on the edges, but more commonly on the common base, and occasionally exhibiting cuneiform summits. The angle of coincidence of its faces measured on the reflecting goniometer, by my friend Doctor Torrey, are 109.23 ; it likewise scratches quartz, is infusible, and scarcely becomes paler by heat. The crystals are in size from the bigness of a pin's head to half an inch in length, the larger, of course, darker, and mostly breaking into splintery fragments, having internally a more or less perfectly conchoidal fracture, and high resinous lustre ; most of the crystals, when slightly rubbed, indeed, present the brilliance of polished steel; the smaller crystals are also so far translucent as to reflect a spendid coloured reflection, little short of that of the diamond. In these cavities they are commonly associated with augite crystals, quartz, hornblende, green felspar, crystals of phosphate of lime, and in two or three of my specimens imbedded mica, arsenical pyrites, and traces of galena. The affinity which these specimens bear to those of Vesuvius, containing Ceylanite is striking to astonishment, and I have even seen the brucite or condrodite, like that of Sparta, from Vesuvius.* No vestige of volcanic fires, more than the pseudo productions of the Missouri, have yet, however, been discovered in the territories of the United States.

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* In the cabinet of Mr. Wagner, junior, Philadelphia.

The CONDRODITE, or BRUCITE, almost peculiar to Sparta, discovered likewise by the celebrated Berzelius, in Finland. accompanied by gray spinelle, is, (according to an unpublished analysis, which I made in 1820,) a silicate of magnesia with an *accidental* portion of fluoric acid and iron The same result has been obtained by Doctor Torrey, from whom I copy the following description of its crystallization. "It occurs occasionally in small four-sided prisms, (of a paler colour than the amorphous masses,) with rhombic bases of 124° and 56° truncated on the acute lateral edges by planes, which form with the contiguous sides of the prism angles of 122° and 114°. The prism is terminated by dihedral summits, whose faces meet under an obtuse angle, and correspond to the acute, truncated edges of the prism. The edges of the summits are oblique to the axis of the prism, but not parallel to each other; or they may be said to be alternately oblique at each extremity."

Haüy, also, by cleavage, had obtained from it a rhomboidal prism ; and Berzelius, who then compared it analytically with his condrodite, concluded it to be identic. The former, with his usual sagacity, referred it to his peridot ; as it does not indeed very materially differ in compositio from olivin or amorphous chrysolite : still the crystallization proves it to be a very distinct species. It is of a bright brownish yellow, bordering on orange, disseminated in masses commonly about the size of a hazle nut, exhibiting more or less of the rhombic form, throughout a white and foliated lime-stone, generally containing scattered hexagonal lamina of plumbago, and, more rarely, bluish pellucid crystals of phosphate of lime. It is imperfectly lamellar in one direction, splintery in the other, with a glassy or almost resinous lustre, the fragments either translucent or almost opaque, and sufficiently hard to scratch glass. Before the blowpipe it is infusible, but with borax forms a glass, though not very readily. Its gravity is a small fraction above 3.

Throughout the valley of Sparta the condrodite is by no means uncommon, but variable in its appearance. The finest and clearest masses are obtained at the town of Sparta.

Though abundant at Franklin, it is here opaque and of a deeper tinge of colour. It occurs in regular subsidiary masses often towards the base of the calcareous beds, and contiguous to foreign infiltrations or veins. A mile south of Franklin furnace it is sometimes imbedded in massive augite, accompanied by mica and fluate of lime: the blue fluate here also forms slender illinitions in the marble. Near to this spot tremolite and small imbedded crystals of white augite? and actynolite occur; short crystals of augite, almost black, like those of volcanic rocks are also now and then visible; a beautiful apple green felspar as indicated by the goniometer, occurs imbedded in the crystalline carbonate of lime, accompanied by perfect crystals of mica, and hexagonal plates of plumbago; this felspar is unusually soft, and almost as fusible as hornblende. A very brilliant pale green hornblende passing into actynolite is often found massive and in implicated crystalline confused crusts over the surface of the calcareous beds.

This hornblende considerably resembles the *supposed* hypersthene of Delaware, recently analyzed by Mr. Seybert, who considered it as hornblende; it however gives the goniometrical measurement of hornblende, while the Delaware mineral, according to the observations of Doctor Torrey, gives by cleavage, a prism which is nearly rectangular, or with angles of 89° and 91°.

It forms, therefore, a new species, which we propose to name MACLURITE, in honour of him who has done so much for American geology and natural science in general.

According to Mr. Seybert, it contains, in the 100 parts: water 1,266; silex 52,166; deutoxide of iron 10,733; manganese a trace; alumine 4; lime 20; magnesia 11,333. From which an obvious affinity to augite presents itself, as it actually differs less in chemical composition from augite, than the two analyses of augite, given in Cleaveland, differ from each other; it fuses also with difficulty; but as the crystalline structure is essentially distinct, it must necessarily be considered as a species destitute of every affinity with hornblende, from which, at the same time it is scarcely distinguishable, except by the cleavage.

In another neighbouring locality, enormous green crystals of augite are found, some at least an inch and a half in diameter, presenting hexaedral or octahedral prisms, with almost equal faces, and terminated by oblique tetrahedral pyramids. These are accompanied, near the junction of the granitine or sienite, with large crystals of felspar, scapolite, or wernerite, and something which borders on spodumene. On the margin of the mill-pond, where some repair was making, a vein of arsenical pyrite, smixed with others resembling the sulphuret of cobalt or nickel, with a substance somewhat like blende, was found, and likewise accompanied by the condrodite.

In another limestone abounding with sphene, dark coloured granules, and minute crystals of augite, there are numerous and generally amorphous, dull grayish blue nodules imbedded, when by goniometrical measurement indicate some variety of fluate. It is nearly quite opaque, but fusible into a white enamel; both externally and internally dull, and minutely splintery or granular in the fracture. It is commonly so much penetrated by the carbonate of lime and titanium oxyd as seldom to present any angle which can be measured. Its hardness is about that of common fluor. It is very feebly acted upon by acids in the cold, but still slowly gives out minute bubbles. When examined it will probably prove the argillaceous fluate of lime, of which I have never seen specimens.

The crystalline calcareous rock, which here alternates with granitines of felspar and quartz, or with beds of sienite, (near to Doctor Fowler's house, the proprietor of the Franklin works,) disappears, and a confluent grauwacke, almost porphyritic, and contemporaneous apparently with the other formations, appears directly overlaid with an horizontal bed of gray minutely granular *secondary* limestone, containing organic remains of the usual shells and corallines, and layers of blackish hornstone or petrosilex. This rock, as well as the grauwacke *beneath*, presents disseminated crystals of blue fluate of lime. In the limestone the cavities are sometimes very numerous, and lined both with pseudomorphous masses and cubes of blue and white fluate and quartz crystals.

Thus we have here before us, as at lake Champlain, the novel and interesting spectacle of an union of every class of rocks, but *passing* decidedly *into each other* as if almost *contemporaneous!* If they are not contemporaneous, how do they happen to penetrate each other by veins? Why do they present similar mineral substances; similar organic remains? Why do the same relicks of plants occur over the anthracite of Rhode-Island, (which is occasionally penetrated even by seams of asbestus,) as over the bituminous coal-fields of Pittsburg and Richmond? Why are the beds of coal, at Richmond, in Virginia, penetrated by *veins* of *granite?*

Lastly, why are the same organic remains found in the *alluvial* limestone of Carolina and Georgia, as those of the Great Calcareous Platform west of the Allegany mountains? Are, in fact, those supposed epochas of time, believed to have intervened between the production of strata any thing more than an imaginary distinction of formations really allied, and as strictly dependent on each other, as the members of the same formation? The grauwacke and *Red* sand-stone, we perceive, contain organic remains; the grauwacke imperceptibly blends with the granitines, sienites, and greenstones of the Highlands. The hornblende rock and its metalliferous deposites unquestionably pass into gneiss; gneiss is foliated granite. Where then are we to seek for permanent distinctions? What is primitive—what transition—what secondary—but the alluvions of rivers and of seas? Of what importance is the inclination of strata, as the uppermost must necessarily be inclined at a decreasing angle?

I am, I must confess, attached to those plausible distinctions of things which tend so importantly to facilitate information and promote instruction; yet I would not wish to submit to the shackles of an imaginary system, or prostrate understanding at the shrine of an ambitious theory. Nature yet presents a wide field for contemplation; there are mysteries yet unravelled—prejudices which blind—systems which for the present impose, and which must ultimately vanish before the test of truth.

ART. XIV.—OBSERVATIONS ON ERGOT:—communicated in a letter to JAMES HAMILTON, M. D. Professor of Obstetrics in the University of Edinburgh, &c. By DAVID HOSACK, M. D. F. R. S. Professor of the Theory and Practice of Physic in the University of the State of New-York.

New-York, June 2d, 1822.

DEAR SIR,

You have probably seen, in the Medical Journals of this country, the various papers which have been published upon the use of ergot, spurred rye, or secale cornutum, in cases of difficult parturition. The testimony with regard to its specific operation upon the uterine system is very abundant, as you will see by adverting to the communications of Dr. John Stearns, Dr. William Moore, and Dr. Dyckman, of this city; of Dr. Prescott, of Massachusetts, and Professor Chapman, of Philadelphia, besides many others, to whom I might refer. In three cases of long protracted labours, wherein all the ordinary resources had been exhausted, the use of instruments excepted, and the patients' strength had become expended, I have, as the result of consultation, prescribed the ergot, giving it in infusion, and in the ordinary doses.

Its effect was within fifteen minutes after its exhibition, to excite the uterus to violent action, which did not cease but with the expulsion of the child. Although no evidence existed, previous to the use of the medicine, that the fœtus was not living, in every case in which it was administered the child was still-born. The same fact I have witnessed in the practice of others. The remark is made by an eminent and judicious practitioner of this city, Dr. William Moore, that the ergot has proved fatal to the child in every case in which he has prescribed it. "It appears," says he, "to be injurious to the child at all times; for in every case, in which I have seen it exhibited, the child has been still-born; and, in the greater part of them, it was not possible to restore it to life." This fact has been conceded even by those who have been most partial to the use of the ergot, and who have been in the habit of prescribing it. It is also true, that, since the introduction of the ergot, the number of still-born children, noticed in the obituary of this city, has greatly increased,—insomuch, that an inquiry has been instituted by the Medical Society of the County of New-York, and a committee appointed to prepare a statement of the facts on this subject. The ergot has been called, in some of the books, from its effects in hastening labour, the *pulvis ad partum;* as it regards the child, it may with almost equal truth be denominated the *pulvis ad mortem*—for I believe its operation, when sufficient to expel the child, in cases where nature is, alone, unequal to the task, is, to produce so violent a contraction of the womb, and consequent convolution and compression of the uterine vessels, as very much to impede, if not totally to interrupt the circulation between the mother and child. It is stated by Dr. Chapman, in his observations* on the effect of ergot, that it has been used, by himself and others, in a hundred cases, without doing harm, in any respect. A similar remark has been made by others. I will not call in question the correctness of these observations; but in the cases referred to, I apprehend the medicine has been employed where the impediment to labour has been very inconsiderable, and the child was readily expelled under its influence, and where perhaps no necessity whatever existed for the use of the medicine, and where nature, left to herself, would have readily accomplished her work.

In the first case in which I prescribed it, the lady, previous to her pregnancy, had been many years married without proving with child, owing to dysmenorrhœa and fluor albus, which we know are oftentimes associated—the latter disease being generally consequent upon the former. Since her delivery, the same painful and sparing menstruation has recurred; and, although nearly three years have elapsed since her lying-in, and she has enjoyed, in other respects, good health, she has not been a second time pregnant.

In the second case, the lady has not menstruated since her confinement, which took place in October, 1820; nor, consequently, I may add, has she proved pregnant. I say consequently, for we know that the exceptions are so rare that impregnation takes place where the catamenia are suspended, that it may be considered as a general observation, that menstruation is necessary to conception. At the same time that I relate the fact of the diminution of the menstrual discharge, in the one case, and the suspension of it in the other, as following the use of the ergot, we should be cautious not to connect these events as, in all cases, cause and effect, until further experience shall justify such inference.

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* See Therapeutics, vol. ii. p. 14.

In the third case in which the ergot was administered, the child was living a few minutes before the medicine was taken. Severe pains immediately succeeded to its use, and were continued, without intermission, until the delivery was accomplished: but, to the astonishment of all present, the child was still-born; and all endeavours to restore it were employed without success.

When, in connexion with the above cases, we advert to the numerous instances recorded of the fatal effects of the ergot upon the fœtus in utero, have we not reason to proscribe, altogether, the use of this medicine in difficult labours, and, as heretofore, in such cases, to place our dependence upon that safer resource, the forceps?

But while, like other potent articles of the materia medica, the ergot proves detrimental under certain circumstances, and is liable to much abuse from its improper administration, nevertheless, by its active operation upon the womb, it promises to be very extensively useful in counteracting many morbid conditions to which that viscus is liable, especially those proceeding from an inactivity in the muscular powers of the uterus, or a lax state of its vessels. Under the former head may be noticed the retention of the placenta, and the disease called physometra: under the latter are, excessive discharges of the lochia, fluor albus, and those uterine hæmorrhages, which proceed from, and are continued by, general and local weakness.

This leads me to the main object I had in view when I commenced this letter, to relate a case of uterine hæmorrhage in which I directed the ergot with the most decided benefit, when other means had been employed in vain.

Mrs. L——, a lady about fifty years of age, of a plethoric but relaxed habit of body, ceased to menstruate about four years ago. Since that time she has evinced the ordinary symptoms of fulness of the blood vessels, that frequently supervene upon the cessation of that function—viz: a sense of distention and pain in the region of the uterus; a fullness of the breasts; oppression of the chest; frequent head ach and giddiness. By blood-letting these have been several times removed; but neglecting this remedy when those feelings returned, uterine hæmorrhage was the result, followed by great loss of strength. In this debilitated state, in which further depletion was forbidden, I had recourse to the various vegetable and mineral astringents, administered internally and externally, but without effect; cold also was applied to no purpose. In this lax state of the uterine vessels, I directed 10 grains of the ergot to be given in substance three times a day. In a few minutes after the exhibition of each dose, it was followed by the uterine nisus, which immediately stanched the discharge. In a few days, by suitable nourishment, and the continuance of the tonics which she had before employed without the effect of restraining her discharges, she recovered her strength.

In the course of the last month, she experienced some return of the hæmorrhage; but, by a few doses of the ergot, the discharge was as immediately restrained as it had been in the first attack.

Seeing these effects of the ergot in producing uterine contraction and in checking hæmorrhage, it has occurred to me, that it promises to become a valuable medicine, in those cases of uterine hæmorrhage that proceed from an attachment of the placenta to the neck and orifice of the womb. The os uteri being dilated, there is certainly much less danger to be apprehended, either to the mother or the fœtus, from the rapid succession of pains, the effect of the ergot, than from the introduction of the hand and turning the child—the practice usually directed in cases of this nature.

In two instances of hæmorrhage, attendant upon the attachment of the placenta over the orifice of the womb, by passing a finger on one edge of the placenta, I ruptured the membranes and discharged the waters ; pains immediately succeeded, and the delivery was speedily accomplished with safety, both to the mother and child. Had I at that time been acquainted with the effects of the ergot, I certainly should have prescribed it, for the purpose of hastening the delivery.

Upon suggesting to Doctor Moore this application of the ergot, he so earnestly concurred in the propriety of the measure, that he at once expressed his determination to give it a trial in the first case of uterine hæmorrhage of this nature that may fall under his care.

Another circumstance occurs to me as favourable to this application of the ergot : that the attachment of the ovum to the orifice, or neck of the womb, never takes place but in those women who have borne many children—in other words, in those cases only where the cavity of the uterus has become enlarged by frequent pregnancies, and consequently accompanied by a correspondent enlargement of the uterine vessels and a propensity to hæmorrhage. In cases of this nature I believe the ergot will prove a valuable resource.

I am, dear sir, with sentiments of great respect,
Your obliged friend and pupil,
DAVID HOSACK.

JAMES HAMILTON, JUN. M. D.

REVIEW.

Art. I. System of Surgical Anatomy, *Part First, on the structure of the Groin, Pelvis, and Perineum, as connected with Inguinal and Femoral Hernia; tying the Iliac Arteries; and the operation of Lithotomy. Illustrated by nine Copperplate Engravings.* By William Anderson, Licentiate of the Royal College of Surgeons in Edinburgh, and Lecturer on Surgical Anatomy in New-York. 4to. pp. 199. New-York. J. V. Seaman.

The author of this work is a young foreigner, who has lately established himself in this city, and is reputed as possessing acquirements of no mean value, in surgical and anatomical knowledge. The production before us will contribute but little, we fear, to enhance the reputation which he may already have acquired. Not that it betrays any particular want of acquaintance with the subject of which it treats, but because it tells us so little that is really new, and which cannot be found in the standard works of the day, expressed, too, in language far more correct and perspicuous. Dr. Anderson may be, and we doubt not is, really a good anatomist, but he is most assuredly a very bad writer. Almost every page of the work before us shocks us with the grossest inaccuracies of style and language—a sin the more unpardonable, inasmuch as it stands unredeemed by any vigour of thought, or strength of diction. That we may not be accused of hazarding random assertions, unsupported by direct proof, we shall offer a few specimens of our author's mode of writing, taken from the "prefatory address to the students composing his class in Murray-street." The very first paragraph begins thus:

"I am induced to occupy the opportunity before us, to *impart* to you upon the nature of this work."—p. vii.

Again;

"Hernia is a disease, the operations for which, are formidable and *big with events* to the patient."—p. viii.

Prophesying the manner in which his work would be received, he tells his students,

"You will have an opportunity, with myself, of observing the manner in which the public will receive this part of the subject; for the critics no doubt will do it the honour of notice, either to decry innovation, or perchance to recommend it: but whichever course they may be pleased to take, *I can here make my own comment.*"—p. ix.

As a specimen of our author's turn for satire, take the following:

"In the chapter upon the operation of lithotomy, I have observed much precision, lest I should lead the student's attention from the anatomy I had previously described; otherwise I might have introduced the subject *by an essay on the doctrines of Bacon*, and apply them to the expediency of possessing anatomical knowledge as the only authority for surgical practice; then afterwards amuse my reader with a ludicrous detail of the freaks of the gorget; preceding it by a history of the several quacks to whom had been assigned the office of the cutting for stone; to point at from among them, to the itinerant Monk of the third order of St. Francis, as having cut the most with success; and afterwards brighten the detail, by recording the parliamentary gift of five thousand pounds sterling to Mrs. Stevens, for rendering the operation altogether unnecessary:—when, by a separate section for the purpose, I might descant upon the various improvements and modifications undergone by the gorget, together with the introduction of the *bistouire cachee*, in exemplification of it having been thought possible to construct surgical instruments that were already endowed with the rational faculty, and requiring only a simple push to accomplish the successful performance of a capital operation."!!!—p. x.

All this, we doubt not, Dr. Anderson might have accomplished; but we really think it very fortunate, both for himself and his readers, that he has left it unattempted. It would certainly be a work of supererogation to assign our reasons, for the purpose of convincing our readers of the correctness of this opinion; and we therefore hasten to give a few more "elegant extracts" from our author's "Prefatory Address."

Descanting on the importance of associations in assisting the memory, he presents us with the following illustration, upon which we believe comment of any kind to be wholly superfluous:

> "If the idea, for example, of the internal abdominal ring should come up in his mind, there would necessarily be *rallied* forth, and by the influence of this ideal attraction, the whole train of neighbouring parts; when immediately will be produced a perfect picture of the relative anatomy of inguinal hernia, almost equal to qualify the surgeon to commence the operation. This picture will be more perfect than any at the end of this work; inasmuch as they make but second hand impressions, while the other is a first edition from the book of nature itself. Solomon must have hinted at a principle like this, when he said, 'Get wisdom, and with all thy getting, get understanding:' he plainly intended to have directed the mind to the true and genuine sources of inquiry, from which only, proper information was to be obtained."—p. xiii.

The most amusing, and perhaps the most original part of this address, is, the description which our author gives of the qualifications of an operating surgeon. "An operating surgeon," he tells us, "should be a *clean* dissector." Again; "As he is frequently called to cut within a *hair's breadth of a patient's life*, it is highly necessary, indeed, that he knows where he *treads*" that is, we presume, when performing an operation a surgeon should look at his feet.

> "But it is necessary also that the surgeon be the medical philosopher; he must be the complete physician; he must have the *brain of a man of science;* for this is the great and high qualification that the operator should possess: he must know when to operate, as well as how to operate; and he must be able, moreover, to anticipate the issue of his patient's case."—p. xiv.

Take it all together, this is certainly a most exalted description of what a surgeon *should* be; and it undoubtedly proves that the views of Dr. Anderson are those of a man deeply impressed with the importance and high responsibilities of his profession. But after all, we cannot help thinking it is rather extravagant, and savours more of "fancy than of fact." That an "operator" should be a clean dissector," we are willing to admit—that he should "know where he treads," too, we grant with equal readiness—that he should know "when to operate, as well as how to operate," we consider indispensable; but when we are told that an "operator" must be the "medical philosopher;" the "complete physician;" and, over and above all these high qualifications and attributes, must have "the brain of a man of science"—we are compelled to demur. Let it be remembered that our author is speaking of the "operator," and not of the philosophical surgeon, whose highest ambition it is, in all cases, to obviate the necessity of an operation, rather than to exhibit his dexterity in the mere butchery of his profession. As far as our observation extends, these two characters are widely different from each other; and facts have convinced us that to be a mere "operator," and a good one, too, requires neither the brain of a man of sense or of science, nor one spark of medical philosophy.

But it is time that we should take leave of our author's Introductory Discourse, which, on a variety of accounts, it is to be regretted he ever published. It is decidedly the worst part of the book. and, we doubt not, has alone decided the fate of the author with more than one reader. The rest of the work, although characterized by the vices and defects of which we have already spoken, is much more reputable, both as to manner and matter. Some portions of it, indeed, we think are peculiarly interesting to the surgical anatomist, and these it shall now be our business plainly and candidly to spread before our readers.

In treating of Hernia, our author gives a description of the crural arch, differing entirely, we perceive, from the commonly received doctrines on this point. As the subject is important, and has given rise to a good deal of discussion, we shall quote, at length, what he says concerning it.

" The CRURAL ARCH. I am perfectly aware, that in the majority of instances, what I am about now to delineate, has the appearance of unity with Poupart's ligament; but from having so often seen them separate, I have ventured at a method of description, which is a little out of the common order, pursued by anatomists. I am not, however, altogether without precedent for this plan, since Mr. Hey, in the first edition of his Practical Observations on Surgery, had adopted it; and moreover, as Mr. Robert Liston has written a book solely upon the subject of the crural arch.

" In the course of my late dissections, I have been frequently led to a very careful examination into the ligamentous structure at the groin; in order that I might satisfy myself of the particular disposition of those tendinous fibres, which form the ultimate seat of stricture in femoral hernia.

" As was to be expected, I found the ligament, of Poupart, running in a straight direction, after the parts connected with it were in a relaxed state; but I also observed, what has been described as the third insertion of the external oblique muscle, to be a considerable ligamentous band, running in an arched course, in the direction of the anterior part of the crural sheath, having its inner attachment from the ligament covering the spinous ridge of the pubis, about an inch from the tuberosity; and outwardly, its fibres seemed to seek connexion with the inferior anterior spinous process of the ilium, by going between the psoas and iliacus muscles, as they come out of the pelvis.

" This crural arch at its inner and upper part, I have seen in many instances, quite distinct from Poupart's ligament, having only a membraneous joining with it, similar to that uniting the two columns of the tendon, of the external oblique, as they go to form the external ring. At about the centre of this arch, however, the fibres become in close contact with Poupart's ligament, and firmly adhere to it, by which their arched course is sustained; yet the fibres are not incorporated here with those of Poupart's ligament, as can be determined by maceration. After passing this centre, the fibres of the crural arch are again separating from those of Poupart's ligament, and are disappearing between the psoas and iliacus internus muscles. Thus we have a ligamentous structure, distinct from Poupart's ligament, distant from the tendon of the external oblique, having attached to it, in close union, the process of fascia transversalis, that descends into the thigh, and prescribes the limits to the mouth of the crural sheath. It is this which Gimbernat found only necessary to divide, to relieve the stricture in femoral hernia ; and it is this, also, to which Mr. Hey, in the first edition of his Surgical Observations, has given the name of the femoral ligament.

" Now, although the separation between the fibres, of what I have here called exclusively, the crural arch ; and those of Poupart's ligament, is not always to be found equally distinct ; yet the arched course of the one set, I believe to be always present, in opposition to those of the other, which from the nature of their attachment, must run in a direct line. That the crural arch is always to be found, is proved by the acknowledged invariable oval shape of the mouth of the crural sheath, when beheld from the abdominal aspect.

" To the fact then : that immediately in contact with and over the anterior part of the mouth of the crural sheath, binding it down from the linea-ileo-pectinea, at about an inch from the tuberosity of the pubis, to the os ilium below the superior spinous process ; are tendinous fibres running in an arched direction, and placed, throughout their course, from being in contact with, to within the distance of an inch below Poupart's ligament ; also that, it is the sharp and inner edge of this, which becomes the principal and ultimate seat of stricture in femoral hernia ;—does this practical inference follow ?—that we are not to expect much relief to the strangulated intestine, in femoral hernia, by elevating the thigh and relaxing the muscles and fasciæ, connected with Poupart's ligament. Hence, we are not to hold in high expectancy, the reduction of a femoral hernia, by the taxis ; for we are told by Sir Astley Cooper, that, ' in the inguinal hernia, the parts are so connected mith muscles, that any relaxation brought upon these, affects the aperture through which the hernia descends ; but in the crural hernia, the seat of the stricture is in parts less connected with the action of muscles, and general relaxation has but little effect upon them' and ' the delay of the operation, which I lamented and condemned, when speaking of inguinal hernia, is to be still more deprecated in the crural ; for death very generally happens earlier in the latter disease than in the former.' And Mr. Lawrence, also, notwithstanding he has disagreed with Mr. Hey's notions, of a femoral ligament or crural arch, distinct from the ligament of Poupart, speaks with some emphasis, in reference to the hope, that is to be entertained, for the reduction of a femoral hernia by the taxis. 'Let the practitioner remember, that the smallness of the mouth of the sac, and the consequent tightness of the stricture, diminish the chance of effecting a replacement of the rupture, by means of the taxis; and consequently, that when the incarceration is completely formed, he should not waste much time in attempts of this description.'"—pp. 61—64.

The following is our author's account of his mode of tying the external Iliac. In some respects it differs from the method usually adopted, and is therefore entitled to the attentive consideration of those who may be called to perform this important operation:

"THE OPERATION FOR APPLYING A LIGATURE TO THE EXTERNAL ILIAC ARTERY.

"The patient is to be placed upon a table, as for the operation for hernia, and his groin being shaved, the surgeon will proceed with a scalpel, to make an incision through the common integuments, commencing at about an inch from the anterior superior spinous process of the ilium towards the pubis. This incision will be carried for a little short of three inches in length, in a line about half an inch above, and nearly parallel to Poupart's ligament; its inner extremity taking a slight curvature upwards, that it may end over the spermatic chord as it passes through the external abdominal ring. The integuments will be dissected downwards, until in a line with the ligament, then upwards for about an inch, throughout the whole course of incision. By this the tendon of the external oblique muscle is to be cleanly laid bare, and afterwards divided for two inches and a half in the direction of its fibres, the incision terminating a little short of the external abdominal ring. This cut will also take a course about half an inch above Poupart's ligament. The tendon is now to be elevated by the handle of the knife, from off the internal oblique muscle, so that the oblique canal may be clearly exposed. This will be found of easy accomplishment, for it is only connected to the parts below by a very delicate reticular membrane, which can be readily broken through.

" The spermatic chord will now be seen taking passage towards the external ring, from under the edge of the internal oblique muscle, about two inches from the pubis. This part is to be lifted on the finger in order that its sheath, the cylindrical process of fascia which proceeds from the ring, may be opened. This will be done by lifting the pellicle with a pair of dissecting forceps, and then cutting slightly with scissors or scalpel. When this is accomplished, the little finger of the right hand will be passed into the opening, and the sheath will direct it to the internal abdominal ring, immediately behind which, will be felt the pulsation of the external iliac artery.

" The peritoneum will not be found much in the way in this operation. Pressing against its angle with the finger a little, will elevate it sufficiently to allow of the artery being secured. It will be remembered that this membrane leaves the anterior abdominal parietes some little way above Poupart's ligament, to turn upon the iliacus and psoas muscles and the iliac vessels. It therefore takes a free course at this part, and does not make an acute angle with itself, as it would if it was continued down, before and behind, to be attached to the back edge of Poupart's ligament.

" An aneurismal needle of silver being now introduced through the internal abdominal ring, it will be insinuated under the artery from its outer side, and moved a little below, that the vessel may be separated from the fascia iliaca, with which it is pretty firmly connected ; as well as from the iliac vein to which it is also attached. The artery will now be elevated through the ring. This will be accomplished more readily by raising the thigh towards the abdomen, and then a silken ligature of sufficient size is to be passed through the eye of the instrument, that it may be drawn under the vessel. The artery will now be tied above the going off of the epigastric and circumflexor ilii vessels, and the ligature, when the part is dressed, is to be allowed to remain without the external wound. One or two interrupted sutures may be applied through the integuments; and when a slight compress and a bandage have been added, the patient may be put to bed.*" pp. 143—145.

It is but just to add, that Dr. Anderson acknowledges that "this method for taking up the external iliac artery, he had seen practised by Sir Astley Cooper."

On the subject of tying the Aorta, Dr. Anderson tells us that he would not hesitate to perform the operation, in any case where it might promise to rescue the patient from immediate death; and he thus describes the different steps of it:

"I would commence an incision beginning over the extremity of the last or twelfth rib of the left side, and continue it through the common integuments as far as the anterior superior spinous process of the ilium. Thus I would lay bare the external oblique muscle, which I would divide for the same extent; this incision would be almost in the course of its fibres. The internal oblique and transversalis muscles I would also divide in the line to the extent of the first incision. These muscles readily retracting, would expose the fascia transversalis, which part I would lift with a pair of dissecting forceps, and open it as directed in the case of a hernial sac. I would then introduce my finger, and extend its division by means of a bistoury as far as the first cut through the common integuments, being cautious to avoid any injury to the peritoneum underneath. I would after this, elevate the peritoneum from the posterior abdominal parietes, by insinuating two fingers, while I supported with the other hand the part raised, until I had separa-

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"* This method for taking up the external iliac artery I have seen practised by Sir Astley Cooper, and in that way I have twice performed the same operation in the living subject. I was pleased to find how much could be accomplished after the tendon of the external oblique was cut through, without the use of a cutting instrument. It is necessary however for me to state, that my two cases terminated unfavourably, from the circumstance of their being in both instances connected with comminuted fracture of the thigh bone, which extended within the cavity of the hip joint; the operations being performed for the purpose of restraining hœmorrhage which attended sloughing, the consequence of the injury; but they had the effect of prolonging the lives of the patients in the one instance for welve, and in the other for eighteen days."

ted as far as the attachment of the mesentery, in the forepart of the lumber spine. I should now see and feel the artery above its bifurcation, and be able, with a properly curved aneurismal needle, to pass a ligature around it, and draw the knot by the contrivance made use of for the extirpation of the tonsils.

" To facilitate such an operation, as remarked in Sir A. Cooper's case, it would be expedient to approximate the shoulders to the pelvis, that the abdominal muscles might be relaxed ; and also, previously, to empty the intestinal canal, that no difficulty should be experienced in elevating the peritoneum, from the pressure of the abdominal viscera."—p. 150.

It will at once be observed by our readers, that the operation proposed by Dr. Anderson differs materially from that performed by Mr. Cooper. In the case of Mr. C. the first incision was made in the course of the Linea Alba, after which it became necessary, of course, to divide the peritoneum. In the plan proposed by Dr. A. this necessity is obviated. And when it is recollected how much our success must depend upon avoiding every possible source of irritation, it cannot but be admitted, that our author has recommended an essential improvement in an operation, always hazardous, but which we do not hesitate to say is, under certain circumstances, perfectly proper and justifiable. We are aware that a different doctrine has been advanced on this subject. Facts, however, are decisive. Our author gives us the details of more than one case, in which, on dissection after death, the canal of the aorta has been found obliterated. Reasoning from such facts, it seems clear that the aorta may be tied without any immediate danger to the life of the subject : and, in those hopeless cases where every other resource of art is unavailing, we can perceive no one sound or even plausible reason that can be urged against its performance.

The remaining portion of our author's work is on *Lithotomy.* This is justly considered one of the most important operations in the whole range of surgery. It has accordingly engaged the attention of many distinguished surgeons ; and so much has already been written on the subject, that originality in any new publication is scarcely to be expected. There is no end, indeed, to what are styled improvements in cutting for stone in the bladder. Almost every surgeon who has had one or two opportunities of operating, conceives himself sufficiently enlightened to propose a new method. Were we disposed to review the operations for stone, from the earliest ages of surgery down to the present day, we should find little difficulty in showing that most of these pretended improvements were only the revival of some old and exploded notions. We are, however, far from believing that lithotomy is susceptible of no further improvement. On the contrary we think there are few surgical operations which require more to be reformed. The different methods now employed for extracting calculi from the bladder, and the different kinds of instruments that are made use of, prove sufficiently the unsettled state of this department of surgery. The *lateral* operation, it is true, is generally practised ; but there are not wanting distinguished lithotomists who employ a different method. The *high* operation is, under certain circumstances, still considered proper ; and, lately, what is called the *recto-vesical* operation has been recommended in strong terms to the consideration of surgeons, as possessing advantages over both the other methods. The recto-vesical operation, to which we here refer, differs from the one performed by Mr. Dupuytren, and several Italian surgeons. The merit of the discovery belongs to Mr. Sanson, who first described the operation as possible ; but it does not appear that he ever performed it. The method, as originally proposed by Mr Sanson, has been adopted by Professor Vacca, of Pisa, one of the most distinguished surgeons of the present day, to whom the profession are indebted for an interesting memorial on the subject, in which he gives an account of several cases that have been operated on by himself and others ; and which cannot fail in directing the serious attention of surgeons to this subject. We regret that our limits will not permit us to give a description of Professor Vacca's method of operating.

Anatomy is the basis of all surgical reasoning, the sole guide of operating ; and in no operation perhaps is correct anatomical knowledge more necessary that in the one now under consideration. We have, it is true, by the invention of instruments gliding along each other by means of buttons and groves, and other awkward contrivances, deprived ourselves of many advantages which might be derived from our knowledge of anatomy. Nothing can be more unsurgical than the sudden, and, as it were, blindfold push of the gorget or cutting director into the bladder, by which that organ may be transfixed, or the instrument driven among the viscera of the pelvis. The operation with the gorget, however, has been approved, and no doubt will continue to be performed. It is perhaps to be lamented, as observed by a celebrated writer, that lithotomy was even brought to that kind of perfection, which may induce ignorant men to venture upon an operation which, even thus simplified, requires consummate skill and knowledge. But we must return to the work of our author.

Dr. Anderson first gives a detail of such anatomical structure, as may claim the attention of the operator in cutting for the stone in the bladder. In this part of his work, we do not discover any thing which requires our particular notice, except the description of what he denominates the "*Ileo-Vesical Fascia.*" As the author attaches much importance to this fascia or pelvic partition, and lays down as a maxim, that it is to be particularly avoided by the cutting instrument, in the operation for lithotomy—we will insert the description of it in his own words.

"ILEO-VESICAL FASCIA. Having observed what was to be seen immediately under the peritoneum, we are now led to give attention to a tolerably dense though semi-transparent membrane, covering the iliacus and psoas muscles, and named fascia iliaca. This will be perceived at the inner side of the crista of the ilium, continuous with the fascia transversalis, and to have boundaries as defined in a former part of the work, while upon the anatomy of hernia. I shall now enlarge upon that description, and describe it as I have traced it in connexion with the urinary bladder.

"We accordingly find the iliac fascia after lying under the iliac vessels, passing into the cavity of the pelvis; by going over the brim from the symphysis pubis to the ileo-sacral articulation, or each side. For some way down it has close adhesion to the side of the pelvis ; and this is as far as the lower part of the symphysis pubis on its forepart, the hole for the transmission of the obturator vessels and nerve in the middle, and as low as the spinous process of the ischium behind. At a line running from these parts it is reflected inwards to have attachment to the side of the bladder—so that it leaves the side of the pelvis by a semi-elliptic course, convex towards the abdomen. We also find it connected to the side of the urinary bladder by a line of attatchment of a similar curvature. This line commences above the situation of the prostrate gland, after having proceeded from the upper surface of the membraneous urethra ; from these parts it rises upwards in conformity to the nature of the curve which is here also convex towards the abdomen : taking this direction, it reaches the opening in the side of the bladder which receives the ureter, and afterwards has a course downwards and backwards, to end at the extremity of the vesicula seminalis.

" There is therefore a loose portion of iliac fascia situated between the side of the bladder and pelvis. This I have called ILEO-VESICAL. It is in breadth anteriorly about half an inch ; becoming gradually wider, is opposite the hole for transmitting the obturator vessels an inch and a half: and then getting narrower, is in the neighborhood of the spinous process of the ischium, less than an inch wide. In looking at the upper surface of this ileo-vescial reflection, we observe that it dips behind the pubis, so that on each side of the symphysis there is a pit or pouch, which is of size to receive the end of the finger. This portion of fascia appears at first view to be of tendinous structure, being more dense and of a different texture from the fascia iliaca covering the muscles in the hollow of the ilium ; which is owing to a circumstance that does not dispute its continuity with the fascia iliaca, and is to be attributed to the tendinous connexion of a part of the levator ani, that will be more distinctly explained when we come to another part of the dissection.

" From this aspect we can see, between the line of attachment of peritoneum to the urinary bladder and that made by the fascia, the muscular coat of this viscus covered by a thin membrane, which is a lamina, (THE SUPERIOR LAMINA OF THE ILEO-VESICAL FASCIA.)

sent up by the ileo-vesical fascia ; and it is by it that this part of the bladder is supported and made equally strong with that defended by peritoneum. This lamina, by sending a process anteriorly from the bladder to the posterior part of the symphysis pubis, forms an anterior ligament to the bladder, which becomes a partition by dipping between the anterior connexions of the ileo-vesical fascia ; and fixing itself to the upper part of the membraneous urethra, aids in forming the two pits or pouches referred to in the last paragraph.

"From this view of the parts, there can be also observed a deep pit behind the bladder, bounded by this fascia, and occasioned by the particular dipping of its posterior part. Into this urine might find access in the high operation for stone ; whence I know not how it could be extricated. Into this I also believe extravasated urine finds a place from the lateral operation by the gorget, in consequence of the manner in which this instrument is sometimes introduced, as shall be noticed when we come to another part of the subject.

"It can moreover be observed at this stage of the dissection, how the peritoneum, as it passes to the rectum, has its angle of reflection just where the ileo-vesical fascia ceases to have attachment to the posterior part of the side of the bladder ; so that it becomes the upper and posterior boundary of this bag into which the urine is likely to fall, from the unguarded use of the gorget in lithotomy. It will now be perceived, that urine thus situated, in however small a quantity, will be in contact with the outer or under surface of this highly sensible membrane. If this is substantiated, I think we have a ready solution to those cases of death after the operation of lithotomy found upon record, and involved in mystery, not having attached to them any rational explanation for the fatal event."—pp. 165—167.

Our author recommends the lateral operation, and concludes his work by a brief review of the different methods that have been adopted for extracting calculi from the urinary bladder. He very justly condemns the use of the gorget, and employs in its stead a beaked knife with a blade of the shape and length of the common probe-pointed bistoury, but of greater strength, proportioned to the parts to be divided. The objections to the gorget are numerous and well-founded. A large number of the best modern surgeons prefer the knife. There may be some choice in the particular kind of knife to be used; but in the hands of one acquainted with the anatomy of the parts, and who has considered the true principles of the operation, the bladder may safely be cut into by almost any kind of knife. A common bistoury, or almost any scalpel will answer the purpose.

But it is time for us to close. We have designedly extended our notice of the present production, because we really felt every disposition to do ample justice to its author. Of the general execution of the work, it has been impossible for us to speak in terms of commendation. At the same time, we are happy to admit that, throughout, it gives evidences of great industry—of an extensive acquaintance with surgical anatomy—and of an honourable and praiseworthy ambition.

In conclusion, we cannot omit giving our unqualified approbation of the mechanical part of the work. The engravings are beautiful; and the whole is got up in a style of elegance quite uncommon in the history of medical publications in this country, and conferring no small credit upon the taste and liberality of the publisher.

ART. IV. AN ESSAY *on the* CHEMISTRY *of* ANIMATED MATTER. By JOHN PATTEN EMMET, of New-York. New-York: C. S. Van Winkle. 8vo. pp. 125. 1822.

AN INAUGURAL DISSERTATION *on the* SANGUINARIA CANADENSIS *of Linnæus.* By FITZGERALD BIRD, of Georgia. New-York: H. Sage. 8vo. pp. 74. 1822.

ALTHOUGH the importance of the medical profession is universally acknowledged, and the necessity of proper regulations obvious,—it is only of late that, in this country, it has been subjected to such restrictions as can, in any degree, tend to its due advancement and respectability. Thanks, however, to the good sense of the community, and to the vigilant care of the proper authorities, we have now laws to protect the lives of our citizens from the mal-practice of pretenders; nor is "any man permitted," as in the time of our colonial historian, Smith, "to set up for physician and chirurgeon." Many individuals, no doubt, still procure a full practice and profitable subsistence, at the expense of the lives of the community; but the career of quackery is evidently on the decline; and the directors of our public councils have the satisfaction already to observe the benign and salutary effects of their well directed patronage.

Among the means which have greatly contributed to these happy results, the College of Physicians and Surgeons established in this city, by the honorable the Regents of the University, has been, by far, the most efficient and powerful. Although a recent foundation, its influence has been widely and beneficially extended: while every year adds to the number of its pupils; the zeal of its professors and the requisitions of its statutes have gradually augmented; and it now stands on a proud elevation of dignity and character.

The publication of an inaugural essay, as evidence of the talents and attainments of the candidate for medical honours, is compulsory in most of the schools of Europe. Nothing but the difficulty of abolishing long established customs can account for the continuance of so absurd a regulation. The expense thus added to education is both oppressive and useless; and experience has shown that the author on the title page is not always even the compiler of the crudities which the work contains. A wiser course has been adopted in America. Inaugural exercises are indeed required by our statutes; but the writer assumes the responsibility of publication, and appears before the tribunal of criticism a voluntary candidate for fame.

The two essays, the titles of which are prefixed to this article, emanate from the school of New-York; and are, in a high degree, honourable evidences of the talents and attainments of the writers. We shall attempt merely a notice of the design and objects of each.

Doctor Emmet begins his dissertation by urging the importance of the study of the principles of vitality; and thinks that they are less difficult to ascertain than is generally supposed, and involved in no greater mystery than is thrown over the other forms of creation.

It is singularly unfortunate for this position, that so little has been done in this science, especially when we advert to the talents which have been applied, from the earliest times, to this favorite theme of philosophers.

Doctor Emmet does not hesitate to assert, that life and its phenomena are the effects of organization : in his own words, "the consequence of material composition." page 13. "Life," says he, in another place, "is nothing else than the *aggregate effect* of numerous material organs endowed with an irritability which subjects them to be constantly stimulated into peculiar and appropriate actions." page 18.

All who have traced the progress of the sciences must be aware of the predominating influence which a fashionable branch of knowledge exercises. In the time of Sir Isaac Newton every phenomenon in nature was accounted for on the principles of the mechanical philosophy, at that time so successfully applied to the explanation of the system of the universe. At all times medicine has been too subservient to the impulse of the other sciences ; and the different theories which have prevailed, form no imperfect index to the current modes of thinking and habits of study.

Chemistry, in its turn, has latterly given a bias and direction to physiological theory. Prescribed by our colleges as a part of professional study, its influence will ever be felt ; and yet we are induced to believe that it will lend its chief aid to the materia medica : nor do we think that it will effect any memorable change in medical practice. The living machine owes allegiance to other than chemical and physical laws.

Life is the triumph of some unknown cause over matter and the laws by which it is governed ; and it is no less philosophical to believe in some superior agency, to which matter is subservient, than to refer the ordinary operations of nature to chemical affinities. The assumption is altogether gratuitous, that life is the result of organization, and all analogy is opposed to such hypothesis ; inasmuch as during life the affinities of matter are modified or suspended; and it is only when life is extinct that the ordinary physical agencies are restored.

This dissertation exhibits, throughout, marks of a vigorous and cultivated mind; and we cannot but regret to perceive so much ingenuity wasted on an unattainable object. Let us be permitted to remind Doctor Emmet, that chemistry is a science eminently practical, and capable of results eminently useful and brilliant; but that the phenomena of life are beyond its reach. It is not a very distant period since philosophers attempted, by the retort and alembic, to convert all metals into gold, and extend life into immortality; and it needs little sagacity to foretel, that these agents, however powerful and adequate to explain the properties of matter, will be as insufficient to explain the properties of life as formerly to prolong it.

On this occasion, we shall avail ourselves of the judicious and accurate observations of that eminent chemist, Dr. Thomson, whose authority none will dispute.

"What is the power which prevents the gastric juice from acting on the stomach during life? Certainly neither a chemical nor mechanical agent, for these agents must still retain the same power after death. We must then, of necessity, conclude, that there exists in the animal an agent very different from chemical and mechanical powers, since it controls these powers according to its pleasure. These powers, therefore, in the living body are merely the servants of this superior agent, which directs them so as to accomplish always one particular end. This agent seems to regulate the chemical powers, chiefly by bringing only certain substances together which are to be decomposed; and, by keeping at a distance those substances which would interfere with, or diminish, or spoil the product, or injure the organ; and we see that this separation is always attended to, even when the substances are apparently mixed together; for the very same products are not obtained which would be obtained by mixing the same substances together out of the body, that are produced by mixing them in the body; consequently all the substances are not left at full liberty to obey the laws of their mutual affinities. The superior agent, however, is not able to exercise an unlimited authority over the chemical powers; sometimes they are too strong for it. Some substances, accordingly, as madder, make their way into the system; while others, as arsenic, decompose and destroy the organs of the body themselves.

"But it is not in digestion alone that this superior agent makes the most wonderful display of its power; it is in the last part of assimilation that our admiration is most powerfully excited. How comes it that the prime substances wanted are always carried to every organ of the body? How comes it that fibrin is always regularly deposited in the muscles, and phosphate of lime in the bones? And, what is still more unaccountable, how comes it that prodigious quantities of some one particular substance are formed and carried to some particular place, in order to supply new wants which did not before exist? A bone, for example, becomes diseased and unfit for the use of the animal; a new bone therefore is found in its place, and the old one is carried off by the absorbents. In order to form this new bone, large quantities of the phosphate of lime are deposited in a place where the same quantity was not before necessary. Now who informs this agent that an unusual quantity of phosphate of lime is necessary, and that it must be carried to that particular place? Or, granting, as is most probable, that the phosphate of lime of the old bone is partly employed for this purpose, who taught this agent that the old bone must be carried off, new-modelled, and deposited and assimilated anew? The same wonders take place during the healing of every wound, and the renewing of every diseased part.

"But neither in this case is the power of this agent over the chemical agents which are employed absolute. We may prevent a fractured bone from healing, by giving the patient large quantities of acids. And unless the materials for new-wanted substances be supplied by the food, they cannot in many cases be formed at all. Thus the Canary bird cannot complete her eggs unless she be furnished with lime.

"As this agent, which characterises living bodies, does not appear to act according to the principles of chemistry, any inquiry into its nature would be foreign to the nature of this work. Physiologists have given it the name of the living or animal principle; and to them I beg leave to refer the reader.

"No sooner is the animal dead, than the chemical and mechanical agents, which were formerly servants, usurp the supreme power, and soon decompose and destroy that very body which had been in a great measure reared by its means"

Doctor BIRD's exercise is devoted to an investigation of the chemical and medicinal qualities of Sanguinaria Canadensis. We think him equally fortunate in the selection of his subject, and in his manner of treating it. It were well if all our native plants, to which popular fame has assigned active medicinal virtues, were subjected to a like examination: we have ever considered subjects of this kind, as furnishing the very best materials for inaugural essays to our candidates for medical honors.

The Sanguinaria Canadensis grows abundantly in North America, from Canada to the Floridas, and is popularly known by many appellations, as the red root, puckoon, blood root. &c. As an article of medicine it has long held a high place, in the estimation of the vulgar. We find it noticed by Schoepf, in 1787; since which time several writers have recommended it in various diseases. With a view to ascertain its chemical qualities, Doctor Bird instituted a series of interesting experiments, which he has detailed at length, and this forms the most valuable part of the essay.

The following extract will furnish our readers with a specimen of the minuteness of Doctor Bird's chemical investigations; at the same that it gives us reason to believe that iodene may prove a valuable addition to the number of best tests for vegetable analysis:

"RESULTS.

"Series first, with the dried root, proves the presence of extract, as is evinced from the precipitate formed by the nitrate of silver. The action of the per-sulphate of iron may arise from some modification of the gallic acid. The extract matter was shown from the precipitate caused by the nitrate of mercury, and from the precipitate induced by the muriate of tin. The extractive principle was also evinced by the opacity occasioned by the muriate and nitrate of barytes, and the muriate of cobalt. The effect produced by the nitro-muriate of platina was owing to extract. The extractive principle was also made evident by the action of the alkalies, and the precipitate induced by the acetate of lead. The tincture of galls formed a precipitate which evinced the presence of cinchonin, and the same inference may be drawn from the effects occasioned by the tartarized antimony and solution of iodene.

"From series second, the presence of gallic acid modified, was made more evident from the effect produced by the sulphate of iron.

"It also contributes to the confirmation of the fact, long since observed by Seguin, viz. that the gallic acid is often unaccompanied by the tannin principle, as in cinchona, crude or roasted coffee, the roots of the strawberry plant, milfoil, and a number of others. The extractive matter was shown by the precipitate induced by the sulphuric acid. The green colour which the infusion acquired by solution of potash, and the changes wrought by ammonia, are all referrible to the presence of extract. The resemblance of the precipitate in colour and specific gravity, as induced by the tincture of galls, to that which this re-agent forms with the cinchona, shows the existence of an analogous principle to the cinchonin of cinchona. No change being caused by the oxalate of ammonia, the absence of lime may be inferred. Extract was also shown by the muriate of tin, nitro-muriate of platina, and by the nitrate of barytes. From the effects of tartarized antimony in this series, the principle which Doctor Duncan calls cinchonin,* would appear to exist. The copious brownish black precipitate which was formed by a solution of iodene, was supposed to arise from the action of cinchonin.

"The third series proves, that alcohol extracts in a greater degree the active properties which exist in the decoction, and in addition, also a resinous matter. The red precipitate formed by the sulphuric acid, denotes abundance of extract. The alterations effected by the nitric acid are referrible to the presence of extract. The cinchonin principle was made more evident by the dense orange precipitate formed by the tincture of galls. The precipitation made by the addition of distilled water demonstrates the presence of a resinous material. Extract was also made evident by the action of chromate of potash. Modified gallic was readily evinced from the dense precipitation caused by the per-sulphate of iron. The abundant precipitate induced by tartarized antimony, leaves no room to doubt the presence of cinchonin. The change produced by the addition of iodene are ascribed to the same principle.

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* See Doctor Duncan's experiments on the Peruvian bark in Nicholson's Journal, 8vo. vol. vi. p. 225.

"Series fourth proves, that the properties of the plant undergo no material change in the temperature of boiling alcohol. Most of the re-agents which were used in the third series, were used in the aqueous solution of the extract; and the effects produced tend to corroborate the results of the preceding experiments. The black precipitate instantly formed by the per-sulphate of iron, readily discovered the presence of gallic acid. The copious and dense precipitate induced by nitrate of mercury was owing to extract. The orange-red precipitate induced by the tincture of galls, evinces that the cinchonin principle in the Sanguinaria undergoes no material change by heat. The same conclusion may be drawn from the effects of tartarized antimony and iodene. The precipitate formed by the addition of muriate of tin was also owing to extract.

"I have stated the results of the simple decoctions, of the alcoholic infusions, and of the alcoholic extract, as taken up by water. In order to discover more satisfactorily the properties of the root as taken up by the latter menstruum, I subjected the tincture (fifth series) to the process of distillation. After the process had been continued, till nearly the whole had come over into the receiver, I employed the usual re-agents, and found all its properties discernible, though in a diminished degree.

"To ascertain how far the properties of the plant may be taken up by the cold aqueous infusion, the sixth series was instituted. It is barely necessary to observe, that the virtues of the recent root are abstracted in a considerable degree by means of the cold infusion. The usual re-agents were employed, and the results were nearly alike. Cinchonin was made evident by the tincture of galls, tartarized antimony, and solution of iodene. The presence of extractive matter was proved by a variety of re-agents. Gum was made evident by silicated potash and the addition of alcohol.

" To demonstrate particularly what peculiar principle in the Sanguinaria, iodene precipitates, I thought it of importance to continue my experiments with the ethereal solution of the root ; in which solution, I found iodene, tincture of galls, and tartarized antimony to produce precipitates of a brownish appearance. Neither of these precipitates can be ascribed to the presence of gum or extractive matter, as both of these principles are insoluble in ether.* The phenomenon, as produced by iodene, is then referrible to the presence of the same principle which the tincture of galls and tartarized antimony precipitate, viz. the cinchonin of Duncan and Vauquelin. To place the point beyond the cavil of criticism, I made a few relative experiments with the cinchona cordifolia, the crocus sativus, and gum arabic. In the alcoholic and aqueous infusions of the bark, a solution of iodene produced brownish coloured precipitates. Upon the saffron (which is known to consist chiefly of the extractive matter) iodene had no effect ; neither did it evince any influence in a solution of gum arabic. That it does not act upon a resinous material, the changes it effected in the decoction of the Sanguinaria at once establish. That it is not fecula, we have only to refer to the precipitate iodene induced in the alcoholic infusion, a menstruum in which starch is insoluble. The difference in the colour of the precipitates as made by iodene in the various experiments, will also establish the point in question. Does not its action upon the Sanguinaria, then, depend upon the presence of cinchonin, or the *Sanguinarin* of the Sanguinaria Canadensis ?

" From the experiments which were instituted, and the results which I have attempted to deduce from them, it will appear that the Sanguinaria Canadensis contains, first, cinchonin ; secondly, extractive matter ; thirdly, a gummy material ; fourthly, a resin ; and fifthly, gallic acid in a state of combination. Whether it is to be ascribed to the existence of extractive matter, an opinion which Doctor Thomson maintains, I do not pretend to determine."—p. 32—40.

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* Brande's Chemistry.

Doctor Bird afterwards enters upon the medical uses of this plant; and from his own observation, he recommends it in various diseases. Among others, in affections of the lungs, and in pertussis As a tonic he has found it of service in rheumatism, after the inflammatory symptoms had yielded to active depletion. From its peculiar principle he believes it will prove efficacious in intermittent fevers, and in diseases of general debility.

Medical and Philosophical Intelligence.

DOMESTIC.

Surgical Operations.—In addition to the notice contained in our last number, concerning an interesting operation performed by Dr. Mott, we have received the following communication, addressed to one of the Editors:

Dear Doctor,

Agreeably to promise, I send thee a few brief notes of my two last cases of operations, which thou witnessed, upon the inferior maxilla, for an osteo-sarcomatous disease of that bone.

On the 30th of March last, I tied the left carotid artery, and removed nearly half of the lower jaw bone, of a married lady, aged about twenty-two years, for an osteo-sarcoma of about twelve months duration. About one year before the operation was performed, she first observed a tumour about the place whence a molar tooth had some time before been extracted. Being at that time pregnant with her third child, the swelling increased but slowly, and was attended with but little pain. Since her confinement, which took place about five months ago, the swelling has increased rapidly, without much augmentation of pain. In extent this swelling was much less than in my first case, but it was rapidly increasing, and had displaced the molar teeth and produced considerable deformity of the face. The operation was attended with the same happy result as in the former instance. No untoward symptoms occurred after the operation; and in about four weeks after, she returned home.

A third case, much more formidable than either of the other two, has again come within my practice. On the 15th of May, I tied the right carotid artery, and removed the lower jaw on the right side, at the joint, and sawed it through on the opposite side at a sound part, after extracting the first bicuspis tooth. The tumour, before the operation, appeared nearly of the size of the patient's head. The diseased mass weighed twenty-two ounces avoirdupois. It is about ten years since its first appearance, and had been regularly enlarging, accompanied with but little pain. The prodigious magnitude of the tumour had for a year or more prevented the patient from taking any solid food, as his tongue could only lay flatwise between the tumour and the left cheek.

He was much exhausted by the operation, but in a short time recovered so as to swallow without difficulty. Most of the drink which he took, after the operation, was conveyed into the fauces through an elastic tube, which he adjusted upon the base of the tongue himself, in a manner so as to prevent, as much as possible, the motion of that organ and the cheeks, in the act of swallowing. Until the fourth day he continued to improve, and showed the most favourable indications of recovery. He was now seized with turns of fainting, difficulty of breathing, accompanied with violent pain in the left side, and very frequent and small pulse, and troublesome cough. He expired towards the evening of the fourth day, in one of his attacks of faintness.

The wounds were dressed on the morning of the fourth day, and appeared to be doing well; that upon the face had, for more than two thirds of its extent, united by adhesion.

The examination, *post mortem*, showed inflammation of the lungs and effusion into the pericardium, as well as a deposit upon the mediastinum. Had it not been for the supervention of this affection of his chest, *I believe he would have recovered.*

I hope to be able to present thee with a circumstantial account of these several operations on the lower jaw-bone, for thy next number.

Believe me to be, with much respect,

VALENTINE MOTT.

JOHN W. FRANCIS, M. D.

Vaccination.—The Editors have received the report of the committee appointed by the Medical Society of Virginia, for the purpose of inquiring into the present received notions of Europe and America, as to the efficacy of the vaccine virus as a prophylactic against the variolous contagion. Its great length prevents its insertion. The decision of so respectable and powerful a society is no small evidence in favour of vaccination.

The society ascribe the cases of failure, which have occurred within their knowledge, principally to the circumstance that the disease itself is so simple as to have been, by consent of parties, entrusted to persons who were ignorant of its pathognomonic symptoms, thereby giving rise to the spurious forms of the disease. They strongly recommend that no patient, labouring under any cutaneous affections, be vaccinated ; and where, from necessity, it has been performed, that it be subsequently tested. They deem the best mode for preserving the matter for use, to confine it in the fluid form between two small plates of glass, and that next to this in regard to efficacy is the scab.

Lunacy.—We shall endeavour to present to our readers, by an early opportunity, an account of the Establishment lately erected near this city, for the management of the Insane—an institution in an eminent degree honourable to the state and country.

Statement of the Patients admitted into, and discharged from, the Bloomingdale Asylum, New-York, from the 1st of January to the 1st of June, 1822—communicated by JAMES EDDY, M. D. Resident Physician.

	Males.	Females.	
Remaining in the house, 31st Dec. 1821,	49	24	73
Admitted since that time,	22	13	35
		Total,	108

Discharged—

	Cured.		Improved.		By Request.		Died.		Total.
	M.	F.	M.	F.	M.	F.	M.	F.	
For January,	4	2	0	0	0	0	2	0	8
February,	2	0	1	1	0	0	0	0	4
March,	1	0	1	0	0	1	1	0	4
April,	5	2	0	0	4	1	0	0	12
May,	3	1	0	0	1	0	0	0	5
	15	5	2	1	5	2	3	0	33

RECAPITULATION.

Discharged since 1st January, 1822—

As cured,	15 Males. 5 Females.
Improved,	2 Males. 1 Female.
By request,	5 Males. 2 Females
*Died,	3 Males.
Remaining 31st May, 1822,	46 Males. 39 Females.
Total,	118

In fourteen of the male and one of the female patients the disease was caused by intemperance.

Suspended animation from Drowning.—A case of this kind, in which some new and efficient modes of treatment were successfully used, we find recorded in a late journal,† by L. W. Smith. Dr. Strong, of Petersburgh, was the physician in whose practice the case occurred, and Dr. Smith gives the following account of the treatment that was adopted.

" He first ordered the body to be stripped of the wet clothes, and at the same time a bed to be prepared with woollen blankets thoroughly warmed with a common bed pan, and in addition to these he directed a kettle of warm water. As soon as the clothes were off, he wiped the body with a flannel cloth, and laid it between the warm blankets. He then observed that appearances were very much against him, and he was doubtful whether he should succeed. Fortunately however, he did not despair, but still persevered in the application of his remedies. While he was preparing the electricity he directed the body to be rubbed with flannel cloths. He then passed the electric sparks twice in succession through the shoulders, and at the same time friction was continued over the whole body, but more particularly about the thorax. The lungs were next inflated by means of the common bellows. All this Dr. Strong observed had no effect. The water by this time was sufficiently warm for his purpose, of which he took about a pint; to this he added a small quantity of brandy. with about a half a table spoonful of Tinct. Cantharides, and gave it per anum. The temperature of the water I do not know. The body, in an erect posture, was next conveyed towards the fire, while the bed was again prepared with warm blankets, and the body returned. The electricity was the second time applied, and was immediately followed by a convulsive sigh. I was at this time rubbing the breast with a flannel cloth, and could perceive a convulsive effort within the thorax which I supposed to be the first returning throb of the heart, and which was repeated three or four times. The lungs were again inflated, and this also was immediately followed by a convulsive effort to breathe, and by an evident palpitation of the heart. The surface of the body was quite warm at this time, and the friction was consequently discontinued. Air was a third time blown into the lungs, and was followed by a discharge of water from the mouth and nostrils. Aqua ammoniæ was applied to the nose, and volatile linament to the breast and back. The person soon began to breathe with tolerable freedom, and was enabled to swallow some warm cordial ; a small quantity of blood was now taken, and he was the next day conveyed home to his friends."

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* One of the above patients died within twelve days after admission; having been received in a state of great bodily debility.

† American Journal of Science and Arts. By B. Silliman, Esq. &c.

Notices of some of the Mineral Springs in the County of Albany—In " A Geological Survey of the County of Albany, taken under the direction of the Agricultural Society of the County," by Professor Eaton, and T. Romeyn Beck, M. D. we find the following notices of some of the mineral springs in that county. One of these springs is rendered peculiarly interesting from the fact of its being highly charged with the sulphate of magnesia, or Epsom salts.

" The most interesting spring observed by us, was that on the farm of Mr. Eliphalet Ackerman, at the village of Coeyman's Landing, mentioned in page 46 of this Report. Some of the water brought to this city, gave the following results with re-agents:

" 1st. Infusion of red cabbage was not affected by it.

" 2d. Nitrate of silver threw down a copious white cloud, which soon settled to a dark brown colour.

" 3d. Oxalate of ammonia threw down a white cloud, not very copious.

4th. Muriate of barytes threw down a dense white cloud, and a very copious precipitate.

" 5th. Carbonate of ammonia gave a dense cloud, with a copious white precipitate. The clear liquor was then filtered, and pure ammonia, on being added to it, produced a copious precipitate.

" 6th. Sulphuric and muriatic acids produced no effect.

" At the spring itself, we found indications of iron, carbonic acid, and sulphuretted hydrogen in small quantities. The bottled water, when brought to this city, seemed to have lost those substances, with the exception, that lime water and barytic water were rendered very slightly turbid.

" We are led to conclude from the above experiments, that the water contains

Sulphuric acid,
Muriatic acid,
Lime,
Magnesia,
Iron,
Sulphuretted hydrogen,
Carbonic acid.

"The result of several repeated experiments proved, that the lime precipitated from 160 grains of the water never exceeds 1½ grains and the desiccated sulphate of magnesia, equals 2 grains. It thus produced the latter salt at the rate of one lb. to every seven gallons of water. On re-dissolving the sulphate of magnesia, an insoluble sediment was found, which was sulphate of lime, and evidently produced by heating the sulphate of magnesia and muriate of lime together, and thus causing a double decomposition. The sediment, however, was in very small proportions, and not to exceed two or three per cent. in any of the trials.

" It is mentioned above, that no iron could be detected in the water brought to Albany. However, on taking the sediment contained in the bottle, and boiling it with sulphuric acid, it gave very strong indications of the presence of iron, both with nut-galls and the prussiate of pot-ash. We infer, that at the spring, the water contains a little carbonate of iron.

" A spring at William M'Kown's, Esq. Guilderland, is strongly impregnated with carbonate of iron and a little sulphuretted hydrogen. It is therefore a good chalybeate.* The same remark applies to the spring at Mr. Solomon's, near Gen. Van Rensselaer's, Watervliet.

" The springs at Mr. Hochstrasser's, Beaver Dam, Bern, in Wendell's Hollow, at Mr. Stephens', Coeymans, and in several other parts of the county, are similar in nature to the Harrowgate springs, at Greenbush."

Report on Absorption.—We have read, with much satisfaction, a very able and elaborate " Report of the Committee of the Academy of Medicine of Philadelphia, on the means by which absorption is effected."† The committee consisted of Drs. Richard Harlan, J. B. Lawrence, and B. H. Coates, and we think they have done credit both to themselves and the association which they represent, by their talent, industry, and candour. They give the following as the results of a very laborious and extended series of experiments, instituted in the presence of several distinguished gentlemen, among whom especially we notice Dr. Chapman, of Philadelphia, and Dr. Brown, of the Transylvania University.

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* Petroleum is found in large quantities floating on the water of this spring.

† The Philadelphia Journal of the Medical and Physical Sciences, No. 6.

"We conceive," say the Committee, "that we have thus established the following facts, in a manner as positive as this kind of evidence admits. 1. That colouring matters are not absorbed by lacteals in the living body. 2. That camphor is absorbed with much irregularity, and in too small quantity to afford proofs of the route of absorption. 3. That assafœtida is more permeating. 4. That prussiate of potass enters by the lacteals and ductus thoracicus. 5. That nux vomica and prussic acid destroy life by their operation on the nerves, and probably in no other way. 6. That the assertion of Magendie, that ink will infiltrate in the living body, is incorrect. 7. That the odours of camphor, assafœtida and mint, infiltrate, through the intestines. 8. That the chemical and odoriferous substances just enumerated, are transmitted into the system with much more delay and difficulty from the stomach, than from the intestinal tube, and with still less from the serous cavity of the abdomen."

It appears very clear from the experiments contained in this report, that the doctrine of the non-absorption of foreign matters, so ably and eloquently defended by Dr. Chapman is quite untenable, and the establishment of this fact does away in a great measure the whole of his reasoning against the humoral pathology.

Elements of Medical Jurisprudence.—Dr. T. Romeyn Beck, of Albany, is preparing for the press, and will shortly publish, a work to be entitled, ELEMENTS OF MEDICAL JURISPRUDENCE. It will be comprized in two volumes octavo, and is intended for the use both of the medical and legal professions.

New Medical Journal.—The first number of a new Medical Journal has appeared a short time since at Cincinnati. It is entitled, "The Quarterly Reporter of Medical, Surgical, and Natural Science; supported by Physicians and Naturalists of the western country; edited by John D. Godman, M. D." &c. Of the general scope and design of the work, some opinion may be formed from the contents, which we copy. "Preface—Medical Education—Observations on Surgical Anatomy—Medical Jurisprudence—De Conceptu adnotationes quædam—Observations on the new

Italian Doctrine of Counter-stimulus—Actions of the Animal Economy—Extraordinary case in Obstetrics, by Wm. H. Luckey—Miscellanea—Intelligence in Anatomy, Physiology, Surgery, Chemistry, &c. &c."

Longitude of New-York.—We copy the following account of the Longitude of New-York, as ascertained by the observation of Professor Renwick, of Columbia College, from the American Journal of Science and Arts, for June 1822.

" I beg leave to communicate, for publication in your valuable journal, my observation on the Solar Eclipse of 27th August last.

The observation was made from the cupola of the College in latitude 40° 42′ 45″ N. as deduced from a number of observations made by my colleague, Dr. Adrain.

The instruments made use of were an Achromatic Telescope by Dollond of four feet focal distance, a box-chronometer by Wingham, (London,) and a pocket chronometer by Morrice, (London.) The rates of both time keepers had been carefully noted for some time previous, and their error was observed by means of transits of the sun on the preceding and following day. This part of the preparation was performed by Messrs. B. & S. Demilt, watch makers of this city, who have erected an observatory, and provided themselves with a transit and an excellent astronomical clock.

With these instruments the times of beginning and end, were as follows, viz.

	days. hrs.
Beginning—Apparent time at New-York.	
1821. August. - - - - -	26.19.37′.32″
End do. do. do. do. do.	26.22.20′.58″

The morning was by no means favourable ; I was in consequence prevented from taking a set of altitudes, that might have enabled me to state the time from my own observations.

The view of the beginning of the Eclipse was as favourable as I could have desired ; the clouds cleared for a few minutes from the sun's face, and my preparatory projection was so far accurate as to enable me to have in the centre of the field of the telescope the exact point of impact.

With regard to the end, the clouds had by that time accumulated to such a degree as to compel me to remove the dark glass from the telescope and I had to contend with alternations of partial observation and intense light.

From the above observation, by a cursory calculation, for the accuracy of which I cannot fully vouch, I make the longitude of the Cupola of Columbia College 74.° 5.′ 11.″ or in time 4hrs. 56.′ 23.″ 4 W. from Greenwich. The mean of a number of Chronometers makes it about 74.° 8′. W. and the Longitude given upon Eddy's Map is 74.° 0.′ 45.″ W."

FOREIGN.

Acupuncturation.—This is a surgical operation, and implies puncturing by means of a needle. The operation is of Asiatic origin, and is employed for the removal of local pain. But, notwithstanding the boasted efficacy of the measure among the Asiatics, it has only recently attracted general attention in Europe, where some favourable reports have been made on it in the public journals. It is considered as peculiarly adapted to diseases of a rheumatic character, unattended by inflammation or excitement of the vascular system. According to Berloiz, of Paris, acupuncture dissipates instantly that state of distress which sometimes attends rheumatism of the external muscles subservient to respiration. "In the space of one or two minutes, a patient whose sufferings drew from him tears, exclaims he is quite well."

In favour of this operation, we have likewise the experience of Dr. Haime, of Tours, who has lately published an interesting memoir on the subject, in the 13th volume of the "Journal Universel des Sciences Medicales," from which we shall extract the following case:

"A woman had suffered for several days with wandering rheumatic pains, which continued daily to increase in violence; there were, however, at all times, fixed pains in the shoulder and in the right arm, which acquired such a degree of intensity by intervals, that the patient could not refrain from crying out. She was in this state when she came to consult me: finding, however, neither alteration in the pulse, nor increase of heat, nor redness of the skin, nor tension, nor swelling in the part affected, I considered the case to be simple rheumatalgia, and passed the needle in the middle of the arm, between the fibres of the triceps brachialis muscle; the place designated by the patient as the seat of the pain. The pain was driven into the fore arm, and the second puncture caused it to descend into the hand, and a third being made in this part, caused it totally to disappear, and the patient said, with delight and astonishment, she was cured; and was so satisfied with this treatment, that she spoke of it to every body."

A treatise on acupuncturation has also been published in England, by Mr. Churchill, Member of the Royal College of Surgeons, in London. The author of this work does not attempt to give any theory respecting the rationale of the remedy, but he speaks very decidedly in favour of its efficacy in "local diseases of the muscular and fibrous structure of the body," to which he has hitherto confined its use. He considers the operation as inapplicable in diseases of an inflammatory character. We will give the details of one or two cases from Mr. C.'s book, which will convey a more correct notion of the nature and effects of the operation, than any general description.

The first case is that of a bricklayer, aged 30 years, who came to Mr. C.'s house, supporting himself by a stick in one hand, and resting the other against the wall, as he proceeded.

"The body was bent at nearly right angles with the thighs, and his countenance indicated acute suffering. He had been attacked, he said, three days before, with darting excruciating pains in the loins and hips; every motion of the body produced an acute spasmodic pain, resembling an electric shock; and the attempt to raise the body to an upright position was attended by such insupportable agony, as obliged him to continue in this state of flexion rather than encounter it by altering his position. There was no more constitutional disturbance than was to be expected from three days and nights of constant pain; the pulse was a little quickened, and the tongue white, but I attributed this derangement to the irritation set up by the pain and loss of rest. I directed him to place himself across a chair for support during the operation, and I immediately introduced a needle of an inch and a half in length into the lumbar mass on the right side of the spine; in two minutes time, I observed that he seemed to rest the weight of his body more on his limbs, and in the next instant, without any inquiry being made, he observed, that he felt his limbs stronger from the 'pain having left his hips.' He next plainly indicated that the disease was lessened, by raising his body; from which he only desisted, by being desired to remain at rest, through fear of the needle being broken. The instrument having remained in its place about six minutes, the patient declared he felt no pain, and could, if he were permitted, raise himself upright; it was then withdrawn; the man arose, adjusted his dress, expressed his astonishment and delight at the sudden removal of his disease, and having made the most grateful acknowledgments, left the house with a facility as though he had never been afflicted."—p. 49.

A young man, employed in a timber yard, whilst in the act of lifting a heavy piece of mahogany, was attacked suddenly with a violent pain in the loins. The weight fell from his hands, and he was unable to raise himself. He was immediately cupped and blistered on the part; but two days passed without much relief.

"On the third day the operation of acupuncturation was performed upon the part of the loins pointed out as the seat of the injury, which, as in the former case, dissipated the pains in five or six minutes, and restored the motions of the back. He returned, however, the next day, with the same symptoms as at first, but in a mitigated degree. A needle was now passed to the depth of an inch on each side of the spine, which, as I expected terminated the disease in a few minutes, and it was with pleasure that I understood the next morning, that the man had gone to his usual employment."—51.

The instrument employed by Mr. C. is a common sewing needle, adapted to a small ivory handle. The mode of introducing the instrument he thus describes:

"The handle of the needle being held between the thumb and fore finger, and its point brought into contact with the skin, it is pressed gently, whilst a rotatory motion is given it by the finger and thumb, which gradually insinuates it into the part, and by continuing this rolling, the needle penetrates to any depth with facility and ease. The operator should now and then stop to ask if the patient be relieved; and the needle should always be allowed to remain five or six minutes before it is withdrawn. This mode of introducing the needle, neither produces pain (or at least very little) to the patient, nor is productive of hæmorrhage, which Dr. Haime says arises from the fibres being separated, rather than divided, by the passing of the needle; the former of which (the absence of pain) is a point in its favour, which few surgical operations possess."—81.

We have now presented to our professional brethren sufficient matter to direct their attention to the operation of acupuncturation—and here our duty ends. We have had no experience of the remedy, and will therefore refrain from offering any opinion concerning its efficacy.

Oil of Croton.—Much interest has lately been excited in Europe by this old and long neglected substance, a single drop of which operates copiously and sometimes violently as a cathartic. This oil is obtained from the seeds of the Croton Tiglium, a native of the East Indies. In the 11th volume of the Asiatic Researches, the Croton Tiglium is thus noticed by Dr. Flemming:

"The seeds of this plant were formerly well known in Europe, under the names of *grana tiglia*, and *grana molucca*. They were employed as hydragogue purgatives; but, on account of the violence of their operation, they have been long banished from modern practice. For the same reason they are seldom used by the Hindu practitioners, though not unfrequently taken as purgatives, by the poorer classes of the natives. One seed is sufficient for a dose, being rubbed with a little rice gruel, or taken in a bit of plantain fruit."

The 25th number of the "Journal of Science and the Arts," contains a valuable paper on the chemical composition of the seeds and oil of the Croton tiglium, by Dr. Nimmo. The author has ascertained that alcohol dissolves a part of the oil, and that both its purgative principle and characteristic acrimony are transferred to the solution, which he considers preferable, as a medicine, to the entire oil. The undissolved portion, which has the appearance of a fixed oil, is destitute of the characteristic acrimony of the original oil.

From a chemical examination of the seeds of this shrub, Dr. N. obtained the following result:

The *shells*, which constitute 36 parts in a 100 of the seeds, possessed no acrimony. 100 parts of the kernels of the seeds yielded

27 of acrid purgative principle,
33 fixed oil,
40 farinaceous matter.

The oil itself consisted of 45 acrid principle,
55 fixed oil.
———
100

Dr. N. considers the alcoholic solution the best form of administering the active principle of the croton oil, and for proportioning the dose to the various circumstances of cases under treatment. The following is the formula he generally employs, together with the directions for avoiding the uneasy feelings which are produced in the mouth and throat:

R. alcoholic solution, ʒ ss.
Syrupi simplicis.
Mucilag. gum. arab. aa. ʒ ii.
Aq. distillat. ℥ ss. M.—ft. haustus.

"After swallowing a little milk, take the draught very quickly, and wash down with repeated quantities of the same diluent." The author has administered this remedy in more than a hundred cases. Slight vomiting was produced in not more than three or four cases, and "in not many more was nausea felt; in all the cases purging was induced in a space of time between half an hour and three hours after taking the medicines: the purgative effects were generally moderate," and rarely accompanied with griping. It was successfully employed in a case of abdominal dropsy, which resisted the use of other remedies. He has also found it a powerful auxiliary to opium in the cure of delirium tremens; and in a case of excessive corpulence, with strong determination to the head amounting almost to apoplexy at times, a few doses of the alcoholic solution produced the most signal benefit.

Piper Cubeba in Leucorrhœa.—Dr. James Orr, of Edinburgh, has communicated to Dr. Duncan, jun. four cases of leucorrhœa cured by cubebs. In the first case, the patient had laboured under the disease for seven months. In the second, the complaint had lasted nearly eight years; in the third, it was of two years' standing; and in the fourth, the discharge had resisted the use of several remedies for six months. In the first three cases, the disease succeeded to abortion. The pepper was administered either in water or milk, or made into an electuary with treacle. The dose was from one drachm and a half and upwards, three times a day, but never exceeded three drachms.—*Lond. Med. & Phys. Journ. May*, 1822.

Sub-nitrate of Bismuth in Intermittent Fevers.—Dr. HENKESEW, a physician at Hildesheim, has been in the habit of prescribing this remedy in agues, for several years. He considers it to be a powerful febrifuge and anti-spasmodic. He exhibits this salt in the dose of four grains, with a few grains of sugar, every two hours.—*Ibid.*

Hydrocyanic Acid.—Mr. BUSH has been in the habit, for some time past, of using, pretty extensively, a form of medicine, the suggestion of which, he says, he owes to our Treatise on Prussic Acid, (*qy.* the second edition?) We mean the emulsion of bitter almonds, in which, as it is well known, prussic acid is naturally present. "Having witnessed," says he, "most beneficial effects from the acid, in its chemical concentrated form, in cases of pulmonary irritation and spasmodic cough, I determined to try a mixture of the bitter almond, as containing the principle in a natural state: this medicine I have given, for the last two years, with considerable success. I have the mixture prepared from a confection made in conformity to the recipe for the Confectio Amygdalæ in the Pharmacopœia Londinensis, only substituting the bitter for the sweet almond; and have given the mixture, thus prepared from a confection of bitter almonds, in doses of an ounce, or an ounce and a half, every four hours; sometimes I have added to each dose a few drops of tinct. scillæ, or tinct. opii camph. I am fully convinced that this medicine is a useful addition to our pharmaceutical preparations: the ease with which it may be at all times prepared, and the certainty of its being in a state of purity, are, I consider, strong recommendations to its use. I have, in a few instances, seen it produce sickness, when given in large doses: where that has been the case, the dose has been lessened, but the medicine continued with advantage. (Frome, April. 1822.)—*Ibid.*

Transfusion of Blood.—MM. Prevost and Dumas have been engaged in a number of microscopic observations and experiments on the nature of the globules belonging to the blood, and on the transfusion of this fluid. They admit the correctness of the opinion of Hewson respecting the form of the globules, and agree with Home in saying, that these central spheres unite themselves in filaments, which differ in no respect from the muscular fibre. They, however, differ from him, where he supposes that the globules experience a rapid change after the emission of the blood from a vessel. These physiologists have found in the milk, in pus, and in chyle, globules or spheres of similar form and dimensions to those of the blood. They consider that these small spheres are surrounded by the colouring matter of the blood. The globules of this fluid are circular in all the mammalia ; their size varies in different animals ; they are smallest in the goat. The colouring matter, which surrounds them, is so thin, that its thickness could not be ascertained by means of the micrometer employed by these physiologists. The globules are elliptic in birds, and vary considerably in size in this class of animals. This variation takes place chiefly in the great axis of the globules. They are elliptic also in all cold-blooded animals.

These gentlemen found that, after bleeding an animal until all organic actions ceased, and injecting, within a few minutes afterwards, the warm blood taken from another of the same species, until a quantity equal to that taken away was restored, the animal gradually revived and took nourishment, and perfectly recovered, if the operation was properly managed.

If the blood injected was taken from an animal of a different species, possessing globules of the same form, but different in dimensions, the animal was very imperfectly revived, and could be rarely preserved beyond six days. The pulse became in these frequent, the temperature fell remarkably if not artificially preserved, while the respiration retained its natural frequency. Immediately after the operation, the dejections became mucous and bloody, and preserved that character until death.

If blood, with circular globules, was injected into the veins of a bird, the animal generally died before the operation was completed, in very violent and rapid nervous convulsions.

Transfusion of blood from the cow or sheep into the veins of the cat, or rabbit, was followed by the recovery of the animal in a number of cases.

The blood of the sheep excited in the mallard (canary) the most violent and rapid convulsions, which were immediately followed by death, according as was observed to follow the injection of the first syringeful in landbirds.—*Ibid.*

Chemical Composition of the Red Rose.—M. F. Cartier has endeavoured to ascertain the chemical composition of the petals of the red rose. M. Gay Lussac had previously refuted the idea that the colouring principle was the result of the presence of the oxide of iron ; since this substance had been found in greater abundance in the petals of the white rose. The experiments of M. Cartier furnish the following substances :—1. A matter of a fatty nature ; 2. An essential oil ; 3. Gallic acid ; 4. A colouring principle ; 5. Albumen ; 6. Tannin ; 7. Soluble salts—carbonate and phosphate of lime ; 9. Silica ; 10. Oxide of iron. The colouring principle was obtained from the docoction of the petals, by means of the acetate of lead, which produced its precipitation. This precipitate being well washed and dried, was afterwards treated by alcohol. *Lond. Med. Repos. No.* 99.

Purification of the Oil of Turpentine.—Dr. Nimmo has employed the following method to purify the oil of turpentine for medicinal use, without diminishing its efficacy, but greatly lessening its disagreeable taste, and its injurious action upon the kidneys. To eight parts of the oil add one part of the strongest alcohol, and let them be well agitated ; in a few minutes a separation takes place ; the oil, unless very impure, falls to the bottom, and the alcohol, having dissolved the impurities, floats at the top. Pour off the alcoholic portion, add an equal quantity of alcohol as before, agitate and separate the liquids. If this be repeated three or four times, the oil becomes nearly tasteless, almost without smell, and when a portion of it is evaporated, it leaves no residue. It is necessary to remark, that, pure as the oil may be rendered, it speedily undergoes alteration, and returns to its original state of greater or less impurity.—*Jour. of Science and the Arts, No.* 25.

On the Existence of Mercury in the waters of the Ocean—By M. Proust.—1. Hilaire Rouëlle remarked, a long while ago, that whenever he purified the crude salt of the custom-house in silver basins, they became covered here and there with those spots which are peculiar to mercury.

2. The same salt, decomposed by sulphuric acid, always gave, in the top of the retorts, small quantities of a sublimate decidedly mercurial.

3. The fact generally known of whitening yellow metals, by putting them for some time into crude or rough salt, added to the preceding results, determined Rouëlle to announce that there was no doubt of the existence of mercury in marine salt.

4. Among the preparations which came to me from Paris to Spain, to furnish the laboratory of the artillery there, were a dozen bottles of fuming muriatic acid, which had been prepared in the laboratory of the druggist, Charlard: all these bottles contained mercury. I at first perceived it, from an amalgam of tin and mercury being left on dissolving some tin in the acid; and afterwards I directly ascertained its presence by purifying the acid in the usual manner, and examining the residuum left; it contained mercury mixed with oxide of iron. It was sufficient indeed to pour a few drops of the proto-muriate of tin into the acid, to precipitate the mercury in powder. Hence, then, mercury incontestibly exists in the custom-house salt of France.

5. In Spain, the government put to sale the rock-salt of the mines of Cordova and Minglanilla. The first time that I purified the salt sold at Madrid, in a silver basin, I remarked the same spots as those noticed by Rouëlle.

6. Having used all the muriatic acid of Paris, I procured some from the manufactory of acids at Cadahalso. That which was sent to me, had been procured by means of calcined clay. It contained iron, and, to my surprise, mercury also. From that time I remarked, in my course of lectures, the singular accordance between the salt of France and Spain in this respect.

The presence of mercury in rock salt is not astonishing, but when it is found also in the salt produced by evaporation from sea water, there is greater difficulty, because it must be supposed to be in solution. There needs not, I think, more facts or better proved ones, than those mentioned to establish with certainty the existence of mercury in the sea water of this period, and that it has existed also in those which, by evaporation or condensation, have given rise to the deposits of rock salt. All the chemists of the last century, but one, speak of the mercury of marine salt, probably from observations analogous to those I have mentioned, and Rouëlle remarked it before me.

There is nothing as yet demonstrated in the origin of rock salt; nevertheless, if it should ultimately be proved that the principal known mines contain mercury, it would be a new demonstration that the waters of the ocean had concurred in producing them; a consequence which has been already drawn from the discovery of potash in the waters of the ocean, and in rock salt.

An experiment I have desired to make for a long time, but for which the opportunity has not yet offered, is to attach a plate of gold, of two or three inches surface, to some part of a ship, where it would be continually plunged in the water. Half an ounce of gold laminated, would be amply sufficient; all that is required is, to ascertain whether, at the end of a long voyage, it had become amalgamated; but the person who would successfully make the experiment must not forget that if the plate is lost, it is probably because it would readily detach itself, nothing scarcely being so fragile as gold penetrated by mercury. As to the expense, I shall readily bear it with pleasure. Application may be made to M. Lucas, agent of the Institute, who will immediately give the value to any person willing to take charge of the experiment.—*Ibid.*

Preservation of Anatomical Specimens.—Dr. MACARTNEY, of the Dublin University, has for some time employed a solution of alum and nitre, for the purpose of preserving anatomical preparations. He finds that it preserves the natural appearances of most parts of the body more completely than spirits or any other fluid heretofore used. The proportions of the alum and the nitre, and the strength of the solution, require to be varied according to circumstances; and, in order to thoroughly impregnate the anatomical preparation, the liquor must for some time be occasionally renewed. The solution possesses such antiseptic powers, that the most putrid and offensive animal substances are rendered perfectly free from fetor by it in a few days. *Lond. Med. Repos.*

Chemical Analysis of Cubebs—By M. Vauquelin.—This article, which has lately become so fashionable in this country in the treatment of Gonorrhœa, has attracted the attention of M. Vauquelin, who has furnished the public with an elaborate analysis of it. We shall select such portions of his description and analysis as we think more particularly interesting to the medical profession.

"Cubebs are the fruit of the piper cubeba (triandria trigynia,) a perennial plant which grows in the Philippine Islands, in Java, Guinea, and the Isle of France.

"The cubebs which were employed for these experiments were presumed to have been collected three years; they were imported by the Dutch, and had been in a warehouse in Paris for 18 months.

"These grains do not all possess the same colour; they are rounded and attached to a stalk: if they are examined after being macerated in water, four coverings are discoverable; the first is fleshy, and softens in water; the second is of a gray colour, and is nearly transparent; the third is thin like the peel of an onion, of a yellowish brown colour; the fourth is a very thin white pellicle, immediately covering the grains, some of which are round, and entirely fill their covering; others are flat on one side, and rounded on the other; others again are wrinkled and covered with fatty matter; and lastly, there are some which are of a white colour.

"Many of these grains contained a white concrete oily matter, and which had every appearance of a crystal. This concrete matter melted in a silver spoon, remained fused, and without emitting any odour."

It results from the extensive analysis of M. Vauquelin, that cubebs contain:

"1. A volatile oil, which is nearly solid,

"2. Resin, resembling that of balsam copaiva.

"3. A quantity of another and coloured resin.

"4. A coloured gummy matter.

"5. An extractive principle similar to that which is found in leguminous plants.

"6. Some saline substances.

"I am desirous that this analysis, upon which I have bestowed some pains, may serve to direct physicians to that employment of cubebs in the healing art, to which they may think them applicable." *Ann. of Philosophy, for March*, 1822.

Medical Antiquities.—Dr. Rochoux has lately discovered a curious monument in the gardens of the capucins of Sarria, a village in the neighbourhood of Barcelona, destined to preserve the memory of a pestilential disease with which this place was afflicted, in 1651. Dr. Rochoux describes this monument to be composed of different groups of individuals, formed from a baked earth and coloured. Of these the largest does not much surpass the fourth part of the common size of the human subject. These groups form a procession which ranges along the exterior of a church. In the midst of a number of assistants, a man appears to fall, struck by the disease. Beyond this central point are observed the sick, the dying, and the dead; the former receiving the kind assistance of, the latter carried by, monks. All these present red blotches, or sores, on the sides, or at the posterior part of the neck, on the arms, and on the limbs. Two of the sick are represented in the act of vomiting; one of these, who are coloured *a dark yellow*, is ejecting an abundance of black matter; another is applying his hand to his mouth, as if in the act of arresting the escape of the matter, with which his distended cheeks appear to be filled. Their eye-lids possess a copperish red colour. A monk appears sustaining the head of one in the one hand, while. with the other, he holds a vessel containing some potion.

On the same monument is represented a small edifice of two floors, covered by mourning drapery and funereal emblems. It is open on one side, and allows the eleven dead monks to be seen, three being placed on the second floor, four on the first, and four on the ground floor. Underneath, the following inscription appears in the Catalan dialect:—

" Noms dels onse religiosos que moriren de la pesta, en lo anii 1652, assistin in lo spiritual y temporal al poble de Sarria essent affligit de tal contagi; y son enterrats bain est panteon: *P. F. Fructuos Rialp, etc. etc.*"

It is apparent from this monument, that a disease ravaged this part of Spain, which was characterized *by yellowness of the skin, by black vomitings, and by red coloured blotches or sores on different parts of the body;* and which appears, from these signs, to have been neither the true plague nor the yellow fever, but in some points to have resembled the epidemic very lately raging in that city. *Lond. Med. Repos. for May*, 1822.

METEOROLOGICAL OBSERVATIONS

FOR MARCH, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer.			Winds.			Rain Gauge.	Weather.
	7 A M	2 P M	9 P M	7 A M	2 P M	9 P M	7 A M	2 P M	9 P M		
1	30 30	30 30	30 30	50	70	57	sw	sw	sw		clear
2	30 20	30 20	30 00	48	52	52	sw	sw	sw	—.17	cloudy
3	29 80	29 80	29 80	43	50	39	w	w	nw		cloudy
4	29 90	30 00	30 10	45	55	40	nw	nw	nw		clear
5	30 20	30 20	30 20	41	50	45	nw	sw	sw		clear
6	30 30	30 30	30 20	45	65	50	sw	sw	sw		clear
7	30 20	30 20	30 20	50	55	40	ne	ne	sw		cloudy
8	30 30	30 30	30 40	36	40	30	ne	ne	ne	—.02	do
9	30 60	30 60	30 60	36	45	28	ne	n	n		clear
10	30 60	30 60	30 60	30	51	31	n	sw	sw		do
11	30 30	30 30	30 20	30	55	51	sw	sw	sw		cloudy
12	29 80	29 80	29 80	45	63	44	sw	sw	sw	—.08	variable
13	29 80	29 80	29 90	45	54	34	sw	nw	nw		do
14	30 20	30 20	30 30	35	52	30	nw	nw	nw		clear
15	30 55	30 55	30 40	32	52	38	nw	nw	nw		do
16	30 20	30 12	30 00	32	46	35	sw	sw	sw		cloudy
17	29 88	29 80	29 80	40	60	45	sw	sw	sw		clear
18	29 66	29 70	29 70	41	62	50	sw	sw	sw		do
19	29 76	29 76	29 70	42	62	38	ne	ne	ne		cloudy
20	29 66	29 66	29 66	42	70	47	ne	ne	ne		do
21	30 28	30 28	30 30	42	65	40	s	s	s		clear
22	30 30	29 99	29 90	40	56	43	se	se	w	—.60	cloudy
23	29 90	30 07	30 26	46	54	37	w	w	w		variable
24	30 38	30 38	30 44	38	56	40	w	w	s		clear
25	30 48	30 48	30 50	42	62	38	ne	se	se		variable
26	30 62	30 62	30 60	40	64	50	se	se	se		clear
27	30 47	30 40	30 16	50	60	50	s	e	se	—.41	do
28	30 00	29 78	29 78	50	60	45	se	s	sw	—.79	cloudy
29	30 00	30 00	30 08	40	50	38	nw	nw	w		do
30	30 13	30 10	30 10	40	50	50	nw	nw	nw		do
31	30 10	30 00	30 00	48	60	50	nw	nw	nw		do

The quantity of rain which fell this month, is 2 inches and 7—100ths.

The weather of the month of March, was for the most part moderate. On the mornings of the 3d, and 8th, a small quantity of snow fell in large flakes, which instantly disappeared. On the mornings of the 17th and 26th, the ground and house tops were white with hoar frost. On the 22d, there fell a little hail, accompanied with rain, the sun was obscured all day; at 11 P. M. lightning in the north. On the 28th. at 3 P. M. strong S. W. wind with a heavy shower of rain, for the space of three quarters of an hour, in which time there fell 54—100ths of an inch of rain. At 6 P. M. a beautiful double rainbow, which lasted about 20 minutes; at half past 7 P. M. thunder and lightning; the accompanying rain amounted to 25—100ths.

METEOROLOGICAL OBSERVATIONS

FOR APRIL, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer.			Winds.			Rain Gauge.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 A M	2 P M	9 P M		
1	29 64	29 60	30 04	50	61	32	nw	nw	nw		cloudy
2	30 20	30 10	30 00	32	50	38	nw	sw	sw		variable
3	29 86	30 00	30 15	41	70	45	sw	sw	sw		cloudy
4	30 44	30 44	30 25	41	59	40	sw	sw	ne		clear
5	30 00	30 00	30 00	45	45	45	ne	ne	ne	—.78	cloudy
6	30 18	30 18	30 18	40	60	42	n	sw	sw		do
7	30 18	30 18	30 18	41	66	55	sw	s	s	—.02	do
8	29 88	30 00	30 24	55	56	45	sw	ne	ne		do
9	30 34	30 30	30 26	41	48	46	ne	ne	ne	—.40	do
10	30 26	30 10	30 00	40	45	45	ne	ne	ne	—.33	do
11	29 77	29 80	30 08	50	69	48	nw	nw	nw		do
12	30 30	30 30	30 26	45	70	45	w	nw	se		variable
13	30 20	29 85	29 78	48	52	60	se	s	s	—.31	cloudy
14	29 60	29 65	30 00	56	72	52	s	w	w	—.03	do
15	30 17	30 17	30 17	42	60	45	nw	nw	nw		do
16	30 17	30 17	30 10	48	55	40	se	se	w	1.02	do
17	30 00	30 08	30 30	50	60	50	w	w	w		do
18	30 30	30 30	30 27	50	60	41	ne	ne	ne	—.31	do
19	30 27	30 27	30 27	40	48	44	ne	ne	ne	—.27	do
20	30 27	30 27	30 30	44	48	45	ne	ne	ne	—.02	do
21	30 37	30 34	30 20	45	60	43	ne	ne	ne		do
22	30 00	30 00	30 16	42	56	42	n	nw	nw	—.14	do
23	30 28	30 30	30 25	42	50	44	ne	ne	ne		do
24	30 10	30 00	30 00	48	70	53	sw	sw	sw		variable
25	30 00	30 05	30 10	50	80	70	se	sw	sw		do
26	30 15	30 15	30 15	70	78	59	sw	ne	ne		do
27	30 15	30 12	30 12	55	71	59	ne	s	s	—.93	cloudy
28	29 88	29 80	29 80	58	70	65	s	sw	sw		variable
29	30 06	30 06	30 20	62	72	64	nw	nw	sw		clear
30	30 30	30 30	30 23	60	73	6[illegible]	nw	sw	sw		do

The quantity of rain which fell in April, is 4 inches and 56—100ths.

This month has been remarkable for its number of rains. On the 5th, at half past 6 P. M., thunder, with very vivid lightning from different parts of the horizon, accompanied with heavy rain. There were evidently two currents of wind—the superior bringing the clouds from S. W., the inferior blowing from N. E. The electric fluid descended in several parts of this city. On the afternoon of the 14th, a gale from the W. with a small quantity of rain from a cloud passing over. On the 27th, at 10 P. M. a gale from the S. with heavy rain. The sun was obscured on the 9th, 10th, 13th, 16th, 19th, and 20th. Thick fogs on the mornings of the 5th, 11th, 14th, 15th, and 30th, which disappeared soon after the sun had risen.

METEOROLOGICAL OBSERVATIONS

FOR MAY, 1822.

Made at the New-York Hospital, by Richard Pennell, M. D.

	Barometer.			Thermometer.			Winds.			Rain Gauge.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 P M	9 PM	7 AM	2 PM	9 PM		
1	30 10	30 08	30 06	62	84	70	sw	sw	sw		variable
2	30 06	30 06	30 20	61	88	75	se	sw	sw		coludy
3	30 25	30 25	30 25	63	75	66	ne	ne	ne		variable
4	30 25	30 25	30 03	58	55	52	ne	ne	ne	—.74	cloudy
5	29 70	29 70	29 78	50	55	47	ne	nw	nw		do
6	29 88	29 96	29 96	46	72	54	w	w	w		clear
7	30 00	30 00	30 00	54	74	64	w	nw	nw		variable
8	29 96	30 00	30 18	60	76	62	nw	nw	nw		cloudy
9	30 20	30 26	30 26	56	71	58	nw	nw	nw		do
10	30 35	30 27	30 27	54	70	58	ne	sw	w		clear
11	30 27	30 27	30 34	56	75	58	w	sw	sw		do
12	30 45	30 47	30 47	55	74	57	sw	sw	sw		do
13	30 47	30 47	30 44	58	75	62	sw	sw	sw		do
14	30 40	30 38	30 32	61	78	61	sw	s	s		do
15	30 16	30 10	30 22	60	75	68	s	s	n	—.18	cloudy
16	30 24	30 24	30 28	62	72	64	n	sw	sw	—.05	do
17	30 35	30 35	30 35	58	68	66	sw	s	s		do
18	30 30	30 26	30 22	62	78	68	s	sw	nw	—.28	do
19	30 20	30 22	30 26	64	80	72	s	s	sw		variable
20	30 30	30 30	30 30	63	75	66	s	s	s		cloudy
21	30 30	30 24	30 16	64	70	64	s	s	s	—.05	do
22	30 06	29 96	29 90	66	85	75	s	w	w	—.20	do
23	30 00	30 08	30 08	70	80	68	sw	sw	sw		do
24	30 22	30 26	30 36	64	75	66	ne	nw	nw		variable
25	30 60	30 60	30 60	60	69	65	sw	sw	sw		clear
26	30 60	30 50	30 46	58	75	60	se	se	sw	—.10	cloudy
27	30 34	30 34	30 26	56	69	62	sw	sw	sw	—.05	do
28	30 12	30 12	30 12	64	85	75	sw	sw	sw		clear
29	30 08	30 12	30 20	72	80	75	sw	se	se		variable
30	30 29	30 29	30 29	65	73	70	se	se	ne	—.07	cloudy
31	30 28	30 28	30 28	62	78	70	ne	s	s	—.02	do

The quantity of rain which fell in May, is 1 inch and 74—100ths.

On the 8th, a few drops of rain from a passing cloud. 16th, at 7 P. M. lightning, with very loud thunder, and a small quantity of rain; 18th, moderate showers; at 7 P. M. thunder. On the 22d, 10 P. M. a gust of wind from the S. W. thunder, &c. The sun was obscured on the 4th, 18th, and 30th. Fog on the mornings of the 7th, 14th, and 15th.

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Art. I. An Historical Review *of some early American Writers on* Croup. By John B. Beck, M. D.

I propose, in the following paper, to give a somewhat detailed account of the early publications which appeared in this country, concerning a disease which in no inconsiderable degree has attracted the attention and elicited the resources of our American practitioners. In attempting this task, I confess that I am mainly actuated by a patriotic motive, to do justice to a class of men whose labours and professional services have never been duly appreciated. Notwithstanding all that has been said concerning the degraded state of medicine in this country anterior to the Revolution, it will be found that even then we could boast of not a few in that profession, who in various erudition, in sound and sagacious views, and in bold and original practice, would not have suffered by a comparison with many of their European brethren. How unjust it would be to judge of these men entirely from the writings which they have left us, need not be insisted upon. Situated as they were, under circumstances peculiarly adverse to literary efforts, it was not to be expected that they would have devoted much time to the labours of authorship. To induce men in any age to make these voluntary efforts, requires, at least, the stimulus of public patronage, and the prevalence of literary taste. These nurture ambition and elicit genius. For a very long period, these incitements, if not wholly wanting in this country, certainly exercised but a limited and feeble influence, and hence it was, that so few original publications made their appearance on this side of the Atlantic. Influenced by the existing wants of the country and the prevailing habits of feeling in the colonies, our physicians devoted themselves almost entirely to the practical discharge of their professional duties, and it was only on special occasions that they were induced to assume the character of authors. In looking over their productions, I have been sensibly struck with the singular excellence of not a few of them, and I have conceived that a review of some of them would be neither uninteresting nor uninstructive. Although by no means the earliest of our medical publications, those relating to croup have been selected for notice at this time, inasmuch as they afford specimens of native talent highly creditable to their authors and honourable to the country.

The first American writer on croup who claims our attention is Dr. Rush. In 1770 he addressed a letter on this subject to Dr. Millar, of London, the author of an essay on asthma and chincough. In this communication, Dr. Rush advances the opinion, that the croup is always a spasmodic disease. In his medical inquiries and observations, published subsequently,* he acknowledges himself to have been in an error in this respect, and adds, that from repeated observations, he was satisfied of there being another species of this disease, to which he gave the name of the *cynanche trachealis humida*, to distinguish it from the *cynanche tra-*

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* In 1794.

chealis spasmodica. The *spasmodic* croup he distinguishes, by its coming on suddenly, and generally in the night—by the frequent and perfect intermissions of its symptoms, without any discharge from the trachea, and by its yielding to antispasmodic remedies, especially the warm bath. The *humid* croup, on the contrary, is distinguished by its coming on *gradually*, and usually in the day time; by its increasing without any remarkable remission or even abatement of the symptoms; by the discharge of phlegm or mucus from the trachea, and by its not giving way to antispasmodic remedies.

Concerning the membrane formed in the trachea, Dr. Rush believed, that it was an accidental consequence of the spasmodic croup, and gives the following account of the way in which he presumed it to originate. "The vessels of the trachea and bronchiæ," says he, "always abound with a thin mucus which is poured into them in proportion as they are irritated by inflammation, or the action of the external air. Children abound with a greater quantity of mucus in those parts than adults; and when it is accumulated in the trachea or bronchiæ in a greater quantity than ordinary, they are unable, on account of their weakness, to bring it up. It is easy to conceive in what manner it is converted into a thin membrane after its more fluid parts are dissipated. We have an analogy of this in the nose. Were the passages of this organ less in our reach, it is probable a membrane resembling that found in the trachea would be found in it every four and twenty hours, especially when it was affected by a cold. We always find the phlegm accumulated in proportion to the time the disorder has continued. In all the cases of membrane mentioned by Dr. Home, the patients never died before the third day, and in many cases, not before the fourth and fifth days." Such was the opinion of our author on this subject in his first publication. He afterwards expressed the belief that the membrane was not present in the spasmodic croup, and that it was an effect only of what he denominated the *humid* species of this disease.

In the treatment of the spasmodic croup, Dr. Rush recommended venesection, when it was connected with pneumonic symptoms ; emetics, purgatives, and antispasmodics, especially the warm bath, opium, assafœtida, and blisters.

In the humid croup, beside bleeding, emetics, and purgatives, he recommends in the most emphatic manner the use of *calomel.* " Our principal dependence," he says, "must be placed upon this medicine. A large dose should be given as soon as the disease discovers itself, and smaller doses should be given every day, while any of its symptoms continue. The bark is scarcely a more certain remedy for intermittents than calomel, when thus administered, in this species of cynanche."

It may not be improper to state in this place, that some difference of opinion has existed as to the true nature of the disease described by Dr. Rush. Cheyne goes so far as positively to deny that it is croup.* That Dr. Rush did not at first very clearly understand this disease is apparent from his own admissions, and the changes which his opinions afterwards underwent; still the majority of writers have agreed with Cullen† in identifying it with croup.

The next writer whom I shall notice, is Dr. Samuel Bard, of New-York, at that time Professor of Medicine in King's College, who in 1771 published an essay, entitled " An enquiry into the nature, cause, and cure of the angina suffocativa, or sore throat distemper, as it is commonly called by the inhabitants of this city and colony." In this production, Dr. Bard speaks of the distemper as one which had lately appeared among the children in New-York, and as being of an uncommon and highly dangerous character. The account of the symptoms corresponds in general with that given of the putrid sore throat by Huxham and Fothergill, and our American writers, Douglass, Colden, Kearsley, and Ogden, with this difference, that more particular mention is made of a symptomatic affection of the trachea and lungs, the existence of which is proved not merely from the symptoms before death, but from the history of three dissections, which present the morbid appearances usually observed in croup.

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* Essay on Cynanche Trachealis, p. 18, Am. ed.

† See Cullen's Nosology.

Dr. Bard next proceeds to compare this disease with the croup, or "suffocatio stridula," as described by Dr. Home; with the "malignant ulcerous sore throat," of Drs. Fothergill and Huxham, and the disease described by Dr. Douglass, of Boston, and concludes that "however they differ in the symptoms of putrescency and malignancy, they do all bear an essential affinity and relation to each other, are apt to run into one another, and, in fact, arise from the same leven, which, as Dr. Fothergill styles it, is a *stimulus of a peculiar nature*, which more or less, according to particular circumstances, generates an acrimony in the humours, and disposes them to putrefaction." According to Dr. Bard, the disease was evidently infectious, but he adds, that the infection did not depend so much on any prevailing constitution of the atmosphere, as upon effluvia from the breath of infected persons. Hence, he says, it would frequently run through a whole family, while the next door neighbour remained unaffected. In the treatment Dr. Bard first speaks of *blood-letting*, and after noticing the opinion of Fothergill, who dissuades from its use, he adds, neither "can I hear of any person who has used it with success or any remarkable benefit; so that I hardly dare venture to prescribe it, but must leave it to the discretion of the physician, until further experience shall confirm its utility or forbid its use."

Calomel was the medicine chiefly relied on; and for this practice Dr. Bard acknowledges himself indebted to Dr. Douglass, of Boston, whose opinion on this subject is quoted at length. Thirty or forty grains, or even a much larger quantity, it is stated, were given to a child of three or four years old, in the course of five or six days, with the most decided advantage; relieving the difficulty of breathing, and promoting the separation of sloughs, "beyond any other medicine."

Oxymel of squills and ipecacuanha, administered so as to produce an emetic effect, are also advised.

Alexipharmics and antiseptics, among which serpentaria, contrayerva, and Peruvian bark, are considered the most powerful, are recommended when symptoms of putrescency appear. In the early stage of the disease, when inflammatory symptoms prevail, they are carefully to be avoided ; and the sole dependence to be placed upon mercurials and diaphoretics. *Seneca snake root* is recommended, "as promoting the attenuating effects of the calomel."

Blisters are advised upon the same principle that they are applied in inflammatory anginas or pleurisies, to relieve the throat and trachea, and to derive the flow of humours from the internal to the external parts.

To me it appears very clear, that the disease described by our author, was not the genuine croup, but the cynanche maligna of nosologists ; and the symptoms of croup, which formed so prominent a feature in some of the cases, were an occasional consequence of the primary disease of the throat extending to the trachea and lungs. In no other way can we account for its infectious character, and for the injurious effects resulting from the use of the lancet in its treatment ; two circumstances wholly irreconcileable with the supposition of its being croup. That some of the prominent symptoms of croup were absent in a large proportion of the cases, appears from the statement of Dr. Bard himself. for he tells us that of seven children who were seized with the disease, three had "no difficulty of breathing." That a symptomatic affection of the trachea should ensue in cases of cynanche maligna, although unequivocally denied by one author,* cannot certainly be considered as very extraordinary. It is a fact confirmed by the testimony of almost every writer on the subject. Mr. Colden states that in the fatal cases of the putrid sore throat, among the last symptoms were " an oppression or straitness in the upper part of the chest, with difficulty of breathing, and a deep, hollow hoarse cough, &c." On dissection, too, he affirms that the lungs were found in a state similar to that of peripneumonic cases.*

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* Cullen's First Lines, note by Dr. Caldwell, Am. ed. vol. 1. p. 257.

Dr. Kearsley also describes among the fatal symptoms, " a suffocatio stridula, (croup,) the effects of a defluxion of acrid ichor upon the lungs."† In like manner Dr. Ogden mentions " a *hoarse cough*, as the last, and generally fatal symptom" of this disease,‡ and so does Dr. Chalmers.§ If therefore it be proper to denominate the disease described by Dr. Bard, croup, no reason can be assigned why the same appellation should not be bestowed upon that described by Colden, Kearsley, Ogden, and Chalmers, universally admitted to be the cynanche maligna. Dr. Bard, himself, probably has given rise to the error which exists on this subject, in maintaining the opinion that croup and cynanche maligna were the same disease.

The sagacious Cullen understood this subject perfectly. " From dissections it appears," says he, " that in the cynanche maligna, the larynx and trachea are often affected in the same manner as in the cynanche trachealis; and it is probable that, in consequence of that affection, the cynanche maligna often proves fatal by such a sudden suffocation as happens in the proper cynanche trachealis; but there is reason to suspect, that upon this subject, *dissectors have not always distinguished properly between the two diseases.*"* The angina suffocativa of Dr. Bard, is accordingly placed in the Nosology of Dr. Cullen, under the head of cynanche maligna, and an impartial examination of the history of the disease justifies the correctness of the collocation.

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* A letter from Cadwallader Colden, Esq. to Dr. Fothergill, concerning the Throat Distemper. Medical Observations and Inquiries of London, vol. 1. p. 215.

† Observations on the Angina Maligna, or the Putrid and Ulcerous Sore Throat, with a method of treating it. By John Kearsley, jr. Practitioner of Physic in Philadelphia. Gentleman's Magazine for 1769.

‡ Letters on the Malignant Sore Throat Distemper. By Dr. Jacob Ogden. Medical Repository, vol. v. p. 97.

§ An Account of the Weather and Diseases of South Carolina. By Lionel Chalmers, M. D. of Charlestown. Vol. i. p. 92.

Nevertheless, it seems proper to assign to Dr. Bard a place among our writers on croup, on account of his having insisted so largely upon the croupy symptoms so frequently attendant upon the putrid sore throat, and because it is probable that in his account he included all the occasional cases of idiopathic croup that occurred during the prevalence of the epidemic which he describes.

The next writer to be mentioned is Dr. Chalmers, of South Carolina. This physician, among the most accomplished of whom our country has ever boasted, beside other valuable productions of his pen, has left us an extensive account of the weather and diseases† of the colony in which he lived. In this work, marked by no common share of learning and observation, are to be found very distinct and accurate notices of our disease. In his chapter on quinseys, he gives us the following exact delineation of it.

" That sort of quinsey, which at once affects the voice with a cough and hoarseness from an inflammation or even an over fulness in the vessels of the *larynx* and *windpipe*, is of all others the most dangerous, and requires the speediest relief. Though a redness or swelling may not appear in the throat, nor the swallowing be at all difficult in this disease, it will nevertheless be known from the heat and pain that are perceived in these parts, when the patient can explain his feelings; the fever and irregularly hard pulse; a hoarseness, which, however, is most distinguishable at the time of coughing or crying; a restlessness and difficult inspiration, and a frequent dry cough, owing to an oppressive sort of stifling, which proceeds from the blood being too much accumulated in the lungs and about the præcordia."* Children under seven years of age, he remarks, are most liable to this complaint.

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* Practice of Physic, vol. 1. p. 193.

† An Account of the Weather and Diseases of South Carolina. By Lionel Chalmers, M. D. 2 vol. 8vo. London, 1776.

In the treatment of this formidable disorder, bleeding, active purgatives, combined with antimonials, blistering the throat, and inhaling the steam of warm water, were the principal remedies upon which reliance was placed. Our author adds, "this complaint should be considered as a most *acute peripneumony*; and therefore it ought to be treated as that distemper usually is in its first stage."

In another part of his work, Dr. Chalmers gives a more extended account of this disease, under the appellation of the "suffocating or catarrhal peripneumony of children." After detailing its symptoms, he adds, "this distemper, in many respects, resembles that sort of peripneumony which succeeds a quinsey; and probably is owing to a slight inflammation, together with an excessive repletion in the vessels of the *glottis*, inside of the *trachea*, and lastly, of the *lungs* themselves."†

Bleeding, purging, blistering, together with the repeated use of emetics, are the means suggested for its removal.

In 1780, Dr. Peter Middleton, a distinguished practitioner of New-York, and Professor of Medicine in King's College, addressed a letter on the croup to Dr. Richard Bayley.‡ In this letter Dr. Middleton endeavours to prove that the *croup* is essentially different from the *sore throat distemper*, and that the method of treating it by calomel and antiseptics, so popular at that period, was founded on erroneous opinions concerning the true nature of the disease. He tells us, that when he first came to this city, in 1752, he found diseases of the throat of very frequent occurrence, and that by the inhabitants, as well as the practitioners of physic, they were commonly considered to be of a highly putrid and malignant character, and the ordinary mode of treatment was by administering large doses of *calomel*, accompanied with antiseptics. Being a stranger to the climate and its diseases, he states that he fell in with the prevailing notions on this subject. Observation and experience soon convinced him of his error, and he relates some impressive instances in which patients were sacrificed, in consequence of the disease having been confounded with the *sore throat distemper*, and the ordinary treatment for that complaint being pursued. The croup he considers a "disease totally distinct from the malignant sore throat: it is not of itself of a nature malignant or infectious, as the putrid sore throat may often be." On dissection, Dr. Middleton found a membrane lining the trachea, from the larynx down to the first division of the bronchiæ, which adhered to the trachea by a viscid mucus. The great bronchial divisions were filled with this mucus. The lungs were found congested with blood.

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* Account of the Weather and Diseases of South Carolina, vol. 2. p. 95.

† Ibid. vol. 2. p. 71.

‡ New-York Medical Repository, vol. xiv. p. 347.

His theory of the disease is summed up in a very few words. He considers it "to proceed from a preternatural flow or discharge of mucus from the glands and vessels within side the trachea, which by the repeated transit of the air in respiration is gradually thickened, and at last condensed into that paper-like form, and which when formed, is difficult to remove, and constitutes not the least part of the danger."

In the treatment of the disease, Dr. Middleton rejected altogether the popular remedies, and in their place had recourse to copious blood-letting from the jugular vein, evacuants of various kinds, together with blisters applied to the throat. A case is related, in which this treatment was found eminently successful, so early as the year 1770. In conclusion, Dr. Middleton admits, that the credit of this mode of treatment was due to Dr. Bayley.

In the following year, (1781,) appeared a letter from Dr. Richard Bayley, of New-York, addressed to Dr. William Hunter, of London.* Unadorned by the graces of composition, this production is nevertheless of a most interesting character, and bears upon it the impress of a vigorous and original mind. What renders it the more valuable, is that it consists in a great measure of cases, illustrating his views concerning the nature and treatment of the disease. Dr. Bayley commences his letter, by detailing some cases of croup and putrid sore throat, which fell under his observation in the year 1774, in which the common mode of treatment by calomel and antiseptics was pursued. They all proved fatal, and from a comparison of the symptoms, with the appearances on dissection, he was induced to believe that the two diseases had not been distinguished by physicians, with sufficient accuracy. In particular, he states that the *hoarse noise* in the putrid sore throat had, no doubt, frequently been mistaken for the "*shrill voice*, which in a great measure is characteristic of the angina trachealis." And concerning the treatment, he adds, " the sudden death of many who have been affected with this complaint, convinced me that a more *determined mode* of treatment was absolutely necessary, in a disorder which appeared *altogether inflammatory*."

In explaining his theoretical views of the disease, Dr. Bayley tells us " that the larynx, aspera arteria, and bronchial pipes have one common membrane, which we are informed by injection consists of little more than an infinity of blood vessels, and consequently liable to *inflammation*, as all vascular parts are. An increased action of these vessels, (as in pleuritic and puerperal fevers,) occasions a preternatural secretion of lymph, which from the ingress and egress of the air becomes condensed, and assumes the appearance of a membrane, and its compactness will depend on the age and habit of the patient, and the state of the atmosphere.

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* Republished in the New-York Medical Repository, vol. xii. p. 331.

" The common opinion is, that those who die of this complaint are suffocated by the membrane's closing the windpipe. Another more respectable opinion is, that a spasm of the muscles of the larynx closes the scene. The circumstances which precede *death* in this disease, compared with those appearances which have regularly taken place in the cases which I have seen *successfully* treated, sufficiently explain the cause of the patient's death from the blood's circulation. To preserve the healthful state of an animal, it is necessary that the whole mass of blood should circulate through the lungs in a given time, and the free admission and expulsion of air contributes to this regular process; the change, also, which gradually takes place in the lungs, seems more directly to account for the swelled face, tumid jugulars, and the full staring eyes, which are symptoms that accompany the progress of this complaint; and add to this, the larynx, aspera arteria, and bronchiæ have been found pervious in every subject I have dissected, while the ramifications have been as regularly filled with a glary mucus."

Considering the disease altogether *inflammatory*, Dr. Bayley directed his curative indications to subdue the excessive action of the vascular system. Repeated bleeding *ad deliquium* from the jugular veins, the free use of tartar emetic and other evacuants, together with blisters to the throat, were the means used to accomplish this object. Several cases are related of the success attending this practice, in the years 1774 and 1779.

From the foregoing review it appears evident, that the subject of croup had scarcely began to attract the attention of the medical men of Britain, when it was investigated in this country, with a zeal and an ability, which must reflect the highest credit upon that period of our medical history.

A brief retrospect of what had been done on this interesting subject, anterior to this time, will exhibit in their true ght the substantial merits of our American physicians. To

Dr. Home, of Edinburgh, it has been customary to award the honour of having given the first perfect description of this disease.[*] The investigations of Michaelis, Cheyne, and Albers, have however rendered it certain that the disease, if not so accurately described, was unquestionably known and understood for a very long period before the appearance of Dr. Home's publication. Thus it appears that even the father of physic was not a stranger to it. And since his time many of the older physicians have noticed it, under various names.[†] That the illustrious Boerhaave was acquainted with the disease cannot be questioned.[‡] And, according to Michaelis, a very accurate history of it was published in 1749, by an Italian physician, of the name of Ghisi.

Notwithstanding all this, it cannot be denied, that Dr. Home called the attention of physicians in an especial manner to this disease, and since his time it has been investigated with increased activity and success. It has indeed been maintained, that by Dr. Starr it had been accurately described so early as 1749. An attentive examination, however, of this author's paper is, I think, sufficient to show, that the disease which he describes partook more of the nature of the cynanche maligna, and accordingly, by Dr. Willan,[||] it is placed under that head. It is evident that the croupy appearances described by Dr. Starr, were symptomatic of the primary affection of the throat. That Dr. Starr had no very clear idea of the nature of croup, is admitted by a writer,[¶] who seems anxious to do him full justice. Dr.

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[*] Published in 1765.

[†] According to Albers, in his learned work on the croup, notices of the disease are to be found in the writings of Hippocrates, Piso, Fernelius, Ballonius, Hildanus, Riverius, Ettmuller, Spigelius, and Severinus. Commentatio de Tracheitide Infantum, &c. &c. p. 37.

[‡] Boerhaave's Aphorisms, No. 801.

[||] On Cutaneous Diseases, vol. 1. p. 247. Am. ed.

[¶] A Treatise on Febrile Diseases. By A. P. W. Philip, M. D. vol. 2. p. 150. Am. ed.

Home, may therefore, with propriety, be considered as the first English writer on this disease. Next to him succeeded Dr. Crawford, who, in 1771, published his "Disquisitio Med. Inauguralis de Cynanche Stridula." In 1778, appeared the elaborate work of Michaelis of Gottingen, entitled, "Dissertatio Inauguralis de Angina Polyposa sive Membrancea." These, I believe, were all the foreign writers, of any character at least, who had appeared at or about the time in which the American physicians just noticed, gave their publications to the world. Dr. Rush published his letter to Millar in 1770. Dr. Bard's essay appeared in 1771. The work of Dr. Chalmers was published in 1776. The letter of Dr. Middleton was written in 1780, and that of Dr. Bayley in 1781. As having been, therefore, among the earliest in investigating this interesting disorder, and certainly at a time and under circumstances by no means propitious to literary enterprizes, no small share of applause is due to the physicians just mentioned. It is not upon this circumstance alone, however, that we ought to rest their claims to consideration. Concerning the nature and mode of treating this disease, they have given us suggestions and observations which have been sanctioned by the more enlightened pathology of succeeding times. On this subject it may not be uninstructive to go somewhat into detail; and,

First, concerning the membrane lining the trachea in croup. By Home, the very erroneous opinion was entertained, that this membrane was the result of a secretion of *mucus* on the inside of the trachea, and that the formation of it was the *cause* of the disease. Michaelis, also, although he controverts the opinion of its being mucus, believed that the membrane, or, as he called it, the polypus, was the *cause* of the disease. On the contrary, Dr. Rush maintained, that the membrane, so far from being the cause, was merely a consequence of the disease, and moreover, that its presence was by no means essential to characterize it. An opinion which has been most unequivocally confirmed by subsequent observations; numerous cases being on record, in which, on dissection, no membrane has been detected.* The views of Dr. Bayley, however, with regard to the nature of the disease, and of the membrane lining the trachea, appear to me to have been more consonant with sound pathology, than those of any writer at that period. From the extract already made from his letter, it appears, that he believed the disease to consist of an inflammation of the inner coat of the trachea and bronchiæ, and that, as a result of this increased action in those parts, an effusion of lymph ensues, which afterwards assumes the appearance of a membrane. It is satisfactory to add, that similar views, in relation to this subject, are promulgated by our latest and most popular writers on this disease.

2. Dr. Bayley, of New-York, is entitled to the credit of having insisted more largely upon the *inflammatory* character of the croup, than any writer who had preceded him. Indeed, the main object of his essay is to establish this point, and at the same time to recommend the efficacy of a corresponding treatment. Liberal blood-letting, it is true, had been practised before this, by Home and others, but Dr. Bayley, upon juster and more comprehensive views of the nature of the disease, pushed this remedy to an extent hitherto unknown. As already stated, he bled from the jugular vein *ad deliquium*, not once, but repeatedly, if necessary, and the rest of his treatment was marked by similar vigour and decision. I am aware that the merit of this practice has not long since been given to another person, by one of the ablest and most eloquent writers of our country. Dr. Chapman, of Philadelphia, in his "Thoughts on the Pathology and Treatment of Cynanche Trachealis, or Croup," after

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* For the elucidation of this point, the medical public is especially indebted to Dr. Jackson, of Boston, and Dr. Chapman, of Philadelphia. See the New-England Journal of Medicine and Surgery, vol. 1. p. 383. and the Philadelphia Journal of the Medical and Physical Sciences, vol. 1. p. 310.

commending the practice of bleeding *ad deliquium*, adds, "this valuable suggestion I derived from Dr. Dick, of Alexandria, one of the most original, bold, and successful practitioners of our country. It has been claimed, I understand, elsewhere, with what justice I pretend not to determine. That however the practice was adopted at least thirty years ago, by this distinguished physician, is unquestionable."* Without wishing to detract in the smallest degree from the general and acknowledged merits of Dr. Dick, it must be admitted that his claims to originality in this practice, are entirely destitute of foundation. Dr. Bayley's publication, recommending it, appeared about ten years before the time alluded to by Dr. Chapman, and it is very evident that he had tested the practice during a pretty long and extensive experience, before that period. It is the more surprising that Dr. Chapman should have fallen into this error, when we find the following in the works of Dr. Rush. "The late Dr. Bayley, of New-York, used to bleed until fainting was induced. *His practice has been followed by Dr. Dick, of Alexandria, and with great success.*"† To set this matter at rest, and at the same time to exhibit a specimen of Dr. Bayley's general mode of managing the disease, I cannot do better than to extract from his letter the account of a very interesting case which fell under his care. "In the latter part of September," says he, "I was sent for to the seat of Peter Stuyvesant, Esq. to visit a servant boy about two years old. In the spring it had been much afflicted with the hooping cough, a little fever pursued it for a month or six weeks, when it became very much reduced. An unexpected alteration, however, took place; the child recovered fast during the two last summer months, and in the beginning of September was in perfect health. About the 20th it grew uneasy in the evenings, and its sleep was frequently interrupted; this continued several evenings and nights successively; a cough, accompanied with hoarseness, attended the feverish symptoms; the cough intermitted, though the hoarseness was permanent from the beginning. When I first saw it, the difficulty of breathing was excessive, and the *hoarseness* accompanied with a *shrill sound*, very distressing to every ear; its face was enlarged and apoplectic, and its eyes altered to a stare; the jugulars were very tense and swollen. In this deplorable situation, it would have been presumptuous not to have been apprehensive for its recovery. It was attempted to bleed the child in the arm, but four ounces of blood could only be obtained, which was judged to be a quantity by no means sufficient to answer any salutary purpose. It was then bled from the *jugular ad deliquium.* Tartar emetic was given in doses to excite vomiting, a blister was advised to cover the larynx and aspera arteria, and directions left to continue the tartar emetic in small doses, so as to keep up a sickness at the stomach, and frequently to throw up a glyster.

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* The Philadelphia Journal of the Medical and Physical Sciences, vol. 1. p. 303.

† Medical Observations and Inquiries, vol. 2. p. 278.

"Dr. Michaelis, Physician General to the Hessian Staff, did me the favour to accompany me in my second visit; we were pleased to find the child had passed a tolerable night, though the hoarseness was still considerable, and its respiration difficult. On examining the blister, it was found to have drawn well, though it had not been applied so high as to cover the larynx and fauces. The child was again bled *ad deliquium* from the jugular; a few grains of calomel were directed to be given with an infusion of senna and manna; this was to be continued until it produced several stools; recourse was then to be had to small doses of tartar emetic, until I repeated my visit. On the succeeding day the child puked twice a thin glary matter with some mucus, which was said to have been exceedingly offensive when first thrown up; but when I first saw it, (fourteen hours afterwards,) it had scarcely any or no offensive smell. The child at this time was in a great measure recovered, the hoarse sound scarcely perceptible, the cough very little troublesome, and without fever. From this date, the symptoms grew more and more favourable, and by a continuance of the same treatment, the child perfectly recovered."

It is impossible to read the foregoing account, without being struck with the exact similarity between the treatment here pursued, and that afterwards recommended by the late Dr. Ferriar.*

3. The practice of administering *calomel* freely in this disease, originated, I believe, exclusively in this country. Who is entitled to the merit of first suggesting it, cannot, at this distance of time, be determined with any degree of certainty. Dr. Chapman claims it for Dr. Kuhn, of Philadelphia, by whom, he says, it "was originally employed in croup, so early as the year 1770."† That this is not correct, must appear very evident, from the preceding history. It is undeniable, that long before this period, calomel was the fashionable remedy in this city, in all diseases of the throat and neighbouring organs. This fact is abundantly established by the writings of Middleton, Bayley, and Bard. Another writer, in treating of this subject, tells us, that Dr. Bayley recommended calomel in this disease, and adds, that "there can be little doubt that this is the *earliest* recommendation of mercury in the curative treatment of hives."‡ This assertion is the more extraordinary, because the same writer informs us, that by Dr. Bard, who wrote ten years before Dr. Bayley, "the free exhibition of mercury is strongly enforced as a proper means of cure."§ Besides, so far from Dr. Bayley even recommending, or approving of this prac-

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* Medical Histories and Reflections, p. 367. Am. ed.

† The Philadelphia Journal of the Medical and Physical Sciences, vol. 1. p. 307.

‡ American Medical Recorder, vol. v. p. 407.

§ Ibid. vol. v. p. 406.

tice, the professed design of his publication was to combat the popular notions in favour of calomel and antiseptics.

If there be any merit at all in the practice, it seems more justly due to Dr. Douglass, of Boston, than to any other person with whose name it has hitherto been associated. It is true, the disease in which this eccentric, but able physician, used the calomel, was the epidemic "sore throat distemper." But it is equally certain, that at that early day, no distinction was made between these two diseases; and Dr. Bard, who insists most unequivocally upon the mercurial treatment in croup, acknowledges himself indebted for the suggestion to Dr. Douglass.* It is of the less importance, however, to settle this question, because there is little probability, that the practice will ever perpetuate the fame of its author, whoever he may be. It is unquestionable, that calomel has lost much of the high fame which it had acquired as a remedy in croup. In Europe, indeed, it seems never to have commanded the entire confidence of the profession, and in this country it has certainly been robbed, in a great measure, of the popularity which it once enjoyed. Notwithstanding the belief prevalent abroad,* few practitioners among us would be found bold enough to rest their chief reliance upon this remedy, in the management of croup. On all hands, it is now conceded, that this disease is of a highly inflammatory character, to be combated by the most efficient agents in our possession, and among these, copious blood-letting, emetics, and purgatives, hold the most conspicuous rank.

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* Nearly a century ago, (1736,) this physician published an account of the epidemic sore throat, in which he extols the virtues of mercury. As a specimen of the medical reasoning of the times, I shall transcribe what he says on this subject. "Any affection of the throat does frequently produce a natural ptyalism. Mercurials, used with discretion, are a kind of specific in such like ulcers, and ulcuscula, and, in fact, here moisten the throat and mouth, stop the spreading of the ulcuscula, and promote the casting off of the sloughs; and as an accessory advantage, the patients being mostly children, destroyed worms. Amongst all the preparations, *calomel* answered best. The gentle vomiting, and few stools that it occasioned in some, did not confound the natural course of the distemper. Turbith produces too strong a revulsion, and the eruption is thereby too much diverted. This distemper did not well bear any other evacuation but mercurials." And in another place he adds, "the dispumation of this acrid inquination of the juices of our distemper, that is, its natural crisis, seems to be by the patent and salutary emunctories of the fauces and skin. In corrosive taints, u. g. venereal and others, a mercurial ptyalism, and sudorific decoction of the woods, answer best; this gave us the hint of promoting the tendency of nature in our illness, by mercurials, and gentle breathing sweats in bed, which, with good management, seldom failed, excepting where the necrosis was irremediable from the beginning."

How calomel should ever have acquired such celebrity in this country as a specific for croup, is a matter of curious and interesting speculation. Dr. Hamilton, of Edinburgh, who tells us that he was induced to give it a fair trial, says, that " the result of his experience has been so very different from that of the American practitioners, that he can only explain it on the supposition, that in different climates, the same disease is relieved in different ways."† This explanation is certainly not satisfactory. There is scarcely a disease apparently less modified in its character by climate, than the croup. At any rate, there seems to be not the slightest difference between it, as it exhibits itself in this country, and in Scotland. A true solution of the difficulty, it strikes me, may be sought for in the fact, that at the period when mercury gained its high reputation in this disease, no distinction was made between it and the putrid sore throat. Both disorders were accordingly treated in the same way, by calomel and antiseptics, and as the sore throat distemper, in which the success of the mercurial practice seems to have been unequivocal, was the more common of the two, it may readily be conceived how the practice might gain upon the confidence of the profession, even though most of the cases of idiopathic croup should be falling victims to it. This explanation acquires additional plausibility when it is recollected, that so soon as a rigid discrimination began to be instituted between the sore throat and the croup, calomel fell at once from the lofty eminence which it had attained as a specific in the latter disease. This fact is very impressively illustrated in the papers of Doctors Middleton and Bayley, both of whom concur in the opinion, that many patients were sacrificed by a blind devotion to the mercurial practice. In addition to this, it may be proper to state, that the writer* who expatiates most largely upon the virtues of this treatment, avows his belief in the identity of the two diseases. That it was not always successful even with him, may justly be inferred from the fact, that the *dissections* which he gives us, were cases in which the symptoms of croup predominated.

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* " In some parts of America, calomel has been employed for many years, as the chief remedy in cynanche trachealis." Observations on the Use and Abuse of Mercurial Medicines. By J. Hamilton, M. D. p. 170. Am. ed.

† Observations on the Use and Abuse of Mercurial Medicines in various diseases. By James Hamilton, M. D. p. 174. Am. ed.

I have been at the more pains to investigate this point, because it is one of those cases in which medical testimony appears strangely contradictory and inconsistent. It would afford a subject of general congratulation were this the only instance of the kind to be met with in the records of medicine. Unfortunately, however, too many of them still exist, and not unfrequently expose to ridicule and contempt, a science, at once profound in its principles, ennobling in its objects, and illustrious in the benefits which it has conferred upon mankind.

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* Dr. Bard.

ART. II. OBSERVATIONS *on the* SECALE CORNUTUM, *or* ERGOT; *with Directions for its use in Parturition.* By JOHN STEARNS, M. D. of New-York.

WE have no information when the *secale cornutum*, or ergot, was first introduced into medicinal use. It may have experienced the vicissitudes of other medicines, whose alternate rise and fall, not being known to succeeding ages, have been repeatedly promulgated to the world as new discoveries. It has been recently supposed to have constituted the grand arcanum of a Dutch accoucheur in Holland, who, in 1747, acquired great fame for his success in obstetric practice. It was subsequently used in France, till it was interdicted by a legislative act, in 1774. Not being subjected to experimental tests, nor prescribed by scientific rules, it was probably exhibited in quantities, and at times, very unfavourable to its safety and success. The injurious effects of such vague practice, and the prevalent opinion that it possessed deleterious qualities, were the probable reasons that caused its rejection.

It was not till the year 1807, that the ergot ever appeared before the public in a form to arrest the attention of medical men. Some years previous to this I was informed of the powerful effects produced by this article in the hands of some ignorant Scotch women in the county of Washington. Determined to try its efficacy, I procured a quantity from a field of rye. My information was such as to impress upon my mind the necessity of extreme caution in my first experiments. The continued influence of this impression upon my subsequent practice, has been a source of much consoling reflection. It has tended to prevent those fatal errors which have so often occurred, and which I trust will be satisfactorily explained in the ensuing remarks.

The frequent recurrence of cases in obstetric practice afforded ample opportunities of executing my design to perfect satisfaction. I gave it in powder and decoction, but the superior efficacy and convenience of the latter soon gave it the decided preference ; and in no instance did I ever give more at a dose than ten grains ; the ordinary quantity was much less. Its sudden and powerful operation upon the uterus, early taught me the necessity of those rules which I subsequently published, and which experience has since amply confirmed. The publication of my letter to Dr. Akerly, in 1807, produced an immense number of applications from remote practitioners. I immediately forwarded to each samples of the ergot, with directions for its use. By these means it was sooner introduced into general practice than I anticipated. Its use was much extended in New-England, by the judicious dissertation of Dr. Prescott, read before the Massachusetts Medical Society, in 1813. The New-England Journal of Medicine and Surgery contained the opinions and experience of several practitioners in that section of country. While it met with general approbation and success, some ascribed to it the fatality of still born children ; and a few, probably influenced by the prejudices of the French, considered it deleterious. The latter opinion was supported by Dr. Mann, Surgeon General of the Northern Army, in a series of essays published in 1813, and intended to prove that the pneumonia typhodes, then prevalent in the country, derived its origin from this source. This was amply refuted by Dr. Henry S. Waterhouse, in a paper published in the New-England Journal.*

The same opinion has been reiterated in some periodical works, and inaugural dissertations, which the young authors imagined they had fortified by experiments upon insects. But it is not by such analogies that important principles in medicine are to be settled. We must refer to the human system as the only correct test for experiments of this sort. And here we find rash practitioners exhibiting the ergot by ounces, to the extent of a quarter of a pound, in less than twenty-four hours ; and in some instances of amenorrhœa and illegitimate impregnation, it has been continued in large quantities for weeks and months, without producing any deleterious effects upon the females, or upon the fœtus in utero. The suggestion of its deleterious influence upon the child, when cautiously given in ordinary parturition, is undeserving of serious notice. Those who exhibit this article in such enormous quantities, and at times and under circumstances unfavourable to its success, and then proclaim it to be " fatal," or " inert," are influenced by other motives than a desire to elicit truth. Tried by such practice, opium, mercury, and antimony, would long since have been condemned and rejected from the materia medica, and the last was actually consigned to this fate, by the civil authorities of France, after its fatal effects had been *fully proved* by the bold empiricism of that day.

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* N. E. Journal, vol. 5. p. 235.

In the accounts which impute the death of still born infants to the ergot, we do not find that minute detail of symptoms, of the quantities given, and of the times and circumstances of its exhibition, which are necessary to enable us to form a correct opinion of the propriety of the prescription. No one can hesitate to believe, that, under certain circumstances, such must be its inevitable effects. And I most sincerely admonish all those who experience such ill success, entirely to abandon its use. On no principle can a continuation of such practice be justified. I can, however, with much satisfaction affirm, that such has not been the result of my experience, and in no instance has either mother or child sustained any essential injury, which would not have been aggravated by its omission. These remarks are fortified by the testimony of many eminent accoucheurs, whose obstetric practice has been extensive, and most of whom have used it for the last fifteen years with the most complete success.* This discordance of opinion can be explained only by supposing a difference in the circumstances, modes, and times, of its exhibition.

It is important, however, to remark, that those cases which indicate the use of the ergot, would have proved hazardous to the life of the mother or child, and peculiarly distressing to both, if it had not been given. When, therefore, these unfavourable effects occur after its exhibition, they are very improperly attributed to the ergot, instead of their real cause, the intrinsic difficulty of the case. It is to prevent these, and to save life, that it ought ever to be prescribed. These effects I have often witnessed, and for this purpose has it frequently been administered by others with equal success. But never, I hope, will this, or any other recommendation of its utility, delude others into the use of it for any other purpose, or under any improper circumstances. If such, in any instance, has been the effect of the opinion that I published in 1807, "that it never produced any bad effects on the patient," I now solemnly retract it. Its bad effects have been too often asseverated to admit of a doubt. I did not then anticipate the abuses to which it has since been subjected. Should these continue, another legislative interdict would be extremely desirable.

To arrest the evil consequences of such practice, and to restrain the use of the ergot, I published in the 7th vol. of the New-England Journal, some plain and important rules, which I deem proper to subjoin, with explanatory remarks, and the addition of those cases in which its use is particularly indicated.

1. "It should never be administered where nature is competent to a safe delivery."

In elucidation of this rule it may be observed, that parturition is one continued process from beginning to end; one portion of which, like the links of a chain, necessarily precedes, in close connexion, that which follows, thus preparing in regular succession suitable changes in the parts concerned. If the interference of art interrupt this order of nature, the chain will be broken, and the whole process may be converted into a difficult labour.

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* N. E. Journal, passim. Med. Rep. 6 vol. N. S. p. 403. Drs. Prescott, Chapman, &c. &c.

2. It should never be administered until the regular pains have ceased, or are ineffectual, and there is danger to be apprehended from delay.

3. "It should never be administered until the rigidity of the os tincæ has subsided, and a perfect relaxation induced."

I am aware, that in my first publication, I intimated that the success of the ergot probably arose from a nausea excited in the stomach, and thence affecting the uterus by sympathy, produced a correspondent relaxation in the rigid fibres of the os tincæ. Subsequent experience has not justified this conclusion, and I have always found it necessary in such cases to premise copious bleeding.

4. "It should never be administered in the incipient stages of labour, nor until the os tincæ is dilated to the size of a dollar."

The success of the ergot is in no case more evident than in the suitable time of its exhibition. Although often given to procure abortion, it does not appear to have succeeded. It also generally fails of complete success when given in the early stage of labour, and before the os uteri is sufficiently dilated and relaxed. The pains induced under these circumstances, often terminate before the labour is fully accomplished. If it is delayed till these favourable changes are produced, its success in promoting the action of the uterus is more certain than tartrite of antimony upon the stomach, or jalap upon the intestines. But while an attentive observation of its effects, under different circumstances, in several hundred cases, have enabled me to predict its precise operation in almost every instance, I feel incompetent to explain why it fails in the one, or succeeds in the other. Its modus operandi is a desideratum, to harmonize conflicting opinions, to prevent, in all cases, its injurious effects, to show why, under certain circumstances, it is inoperative, and under others, powerfully efficacious. Dr. B—— informed me, that he once gave it to a woman before any symptom of labour had appeared, to enable him to perform a journey which this case delayed. In one hour labour actually commenced, and regularly proceeded through its different stages to a safe and expeditious delivery. Had the presentation in this case been wrong, and other circumstances unfavourable, his premature prescription might have been fatal to both mother and child. I therefore seriously admonished him never to repeat it in a similar condition.

5. "It should never be administered in any case of preternatural presentation that will require the fœtus to be turned."

The efficacy of the ergot is fully proved by the peculiarity of pains which it induces. From five to twenty minutes we first discover a bearing down effort of the patient. This gradually increases without the least intermission till the delivery is completed. During such an uninterrupted action of the uterus, all efforts to turn the fœtus must be unavailing and hazardous.

6. "It should never be administered during the continuance of one labour, in larger quantities than thirty grains by decoction in half a pint of water." A table spoonfull of this given every ten minutes, generally succeeds better than a larger dose. While this quantity produces its most favourable effects upon the uterus, it does not affect the stomach with nausea or vomiting, which sometimes interrupts its successful operation.

Three grains, with a grain of opium, steeped in a gill of water, and a tea spoonfull given every ten minutes, have succeeded in reproducing the interrupted pains of regular labour. I have, therefore, generally preferred it in this form, as being perfectly safe, and exempt from the objections to the incessant forcing pains induced by larger doses.

By a due observance of these negative rules, it will be perceived, that but few cases can occur that will require the ergot; and for several years past I have not found it necessary to administer it oftener than in one for every thirty that I have attended. But so important do I consider it in certain cases, that I always have it ready for use on the occurrence of any emergent symptoms that may render it immediately necessary.

I will now proceed to consider those indications which render its exhibition necessary and important.

The ergot is indicated, and may be administered,

I. When, in lingering labours, the child has descended into the pelvis, the parts dilated and relaxed, the pains having ceased, or being too ineffectual to advance the labour, there is danger to be apprehended from delay, by exhaustion of strength and vital energy from hemorrhage, or other alarming symptoms.

II. When the pains are transferred from the uterus to other parts of the body, or to the whole muscular system, producing general puerperal convulsions.

After premising copious bleeding, the ergot concentrates all these misplaced labour-pains upon the uterus, which it soon restores to its appropriate action, and the convulsions immediately cease. A remarkable instance of its efficacy in these affections, is contained in a letter which I received from Dr. Henry S. Waterhouse, of Franklin county, and is too interesting to be omitted in this place.

"Mrs. L. H., of nervous temperament and delicate habit, aged 19, was, on the 24th of June, 1814, seized with the usual precursory symptoms of parturition. I found her affected with wandering pains of the back and abdomen, some throbbing pain of the head, and a tense pulse, though natural in frequency. The loss of 15 ounces of blood, with fomentations to the abdomen, and a dose of opium, gradually gave her relief, and at evening she fell into a quiet and refreshing sleep. The next morning I was sent for in haste, and was informed, that after a quiet night, she discovered in the morning some symptoms of derangement. She complained of wandering pains in the abdomen, and of a throbbing sensation in her head. These symptoms increased till the most horrid form of puerperal convulsions was brought on, that I ever witnessed. She was constantly muttering things in an incoherent manner; her eyes were rolling from side to side, and turning up in their sockets. She had so frequently bitten her tongue, that the blood was flowing profusely from her mouth; her extremities were of a deadly coldness, and the violent spasms and contractions of the muscles of her limbs, back, abdomen, neck, and lower jaw, were truly alarming. The pulse was natural, but less frequent than in health. With much difficulty her lower extremities were immersed in warm water, and large quantities of the tinctures of opium and assa fœtida were forced down. Her abdomen was fomented, and her extremities smartly embrocated with stimulating applications, &c. but all to no purpose. There was no hemorrhage, but from the condition of my patient, it was impossible to make that accurate examination per vaginam that I wished. I could, however, ascertain that the os uteri was in a small degree dilated. The circumstances were so urgent that I could not defer the use of means till I could procure a consultation. Her strength was rapidly wasting, pulse small and frequent, breathing laborious, and countenance ghastly. The ergot presented itself to my mind as the only probable means of saving her life. I mixed thirty grains in a small quantity of warm water, and gradually insinuated a table spoonful between her teeth, worked it into her mouth, and in two or three minutes she had swallowed it. The effects were almost instantaneous and truly astonishing. Her spasms gave way, the operations of her mind became regular, and she awoke, as she supposed, from a disturbed and painful sleep. She complained of much weariness. A strong cup of tea was given her with some light nourishment, and she soon fell into a quiet sleep. In the evening following, true and forcible labour-pains came on, and I delivered her in a short time with perfect safety."

III. When in the early stages of pregnancy, abortion becomes inevitable, accompanied with profuse hemorrhage and feeble uterine contractions.

IV. When the placenta is retained from a deficiency of contraction.

V. In patients liable to hemorrhage immediately after delivery.

In such cases the ergot may be given as a preventive, a few minutes before the termination of the labour.

VI. When hemorrhage or lochial discharges are too profuse immediately after delivery, and the uterus continues dilated and relaxed without any ability to contract.

I have thus exhibited a general view of the errors often committed in prescribing ergot, of the unfavourable results of such practice, of those cases in which it never ought to be administered, and of the indications which render its exhibition necessary and important. These remarks are derived from actual experience in several hundred cases, and are confirmed by those whose observations have been the most extensive and correct. While there is reason to suspect the influence of prejudice upon the minds of some who oppose its use, their own statements generally admit their very limited opportunities for witnessing its effects, and in some instances, while using it in their first experiments, on which their opinions were founded, that they grossly deviated from every direction calculated to ensure success.

While the frequent occurrence of such abuses is to be deplored, much satisfaction may be derived from the reflection that a prudent and judicious use of this article, has in a great variety of instances contributed to save the lives of the mother and child. That such will continue to be its effects when directed by a discreet, judicious, and experienced practitioner, we have the most satisfactory reason to infer from past experience, and from the peculiar properties and operation of the ergot.

ART. III. CASES OF HYDATIDS *in the* BLADDER *and* LIVER. *By the late* JAMES LOW, M. D. *of Albany*, (N. Y.)*

CASE I. *Hydatids in the Bladder.*—Mr. S———, aged 29, of a thin spare habit of body, made a voyage to London in the year 1810. He sailed from New-York late in the autumn of that year, and arrived on the coast of England about the latter end of December, at which time the weather was not only very boisterous, but excessively cold, to all of which he was constantly exposed. While beating about in the English channel, he was seized with what he termed a difficulty of passing water (or strangury.) This continued till he arrived in London about the second week in January, 1811, when from the use of diluents, anodynes, &c. he was partially relieved. He consulted some of the first physicians in London, who advised nothing more than what has been above specified.

In his voyage from Liverpool, in 1811, to New-York, he had a second severe attack, which was gradually relieved by the same means he had formerly used. About two months after his arrival in this place, on a third attack, he consulted me. I found him labouring under a degree of strangury, but of a kind totally dissimilar from any I had before seen.

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* A friend, to whom we are indebted for this communication, informs us, that the cases are selected from the records of a private medical society formed at Albany some years since, for the purpose of mutual improvement. Those who were acquainted with Dr. Low, in the prime of health and mental vigour, will readily appreciate his value in such a body; and the surviving members conceive that they will offer the best tribute of respect to his memory, in communicating some of the results of his observations to the public. B.

I ordered him, however, large doses of nitrate of potash and opium, with plentiful draughts of barley water. During the use of these medicines, his water suddenly stopped, and it appeared as if some immoveable body plugged up the calibre of the urethra. On his pressing at the point of obstruction, the obstructing body appeared to give way, and the water again to flow freely. Being informed of this circumstance, I was induced to examine the water, and found floating in it a number of scaly membranous bodies, and on closer inspection, a number of very small, but perfectly entire hydatids.

I immediately ordered him the tinct. oxymur. ferr. in doses of *gtt.* xii. four times a day, from which period, enormous quantities of hydatids were daily passed, some entire, some broken, for the first ten days; after that time they gradually diminished, and in a few days, totally disappeared. The patient continued the medicine for about a month, and has since been perfectly free from disease. It would be almost superfluous in me to say, that my *methodus medendi* was derived from the excellect treatise of Mr. Carmichael on cancer, and other anomalous diseases. From the effects of iron in this, and many other analogous cases, much importance is added to the practice as well as theory of Mr. Carmichael.

Case II. *Hydatids in the Liver or Ovarium.*—In the summer of 1808, I visited Mrs. K———, aged 73, of a large and athletic stature, sanguine temperament, and firm and vigorous constitution. She informed me that for the last three years, she had been afflicted with a dull heavy pain in the right side, and that within a short time she had attended to all the concerns of the family herself, which must have been considerable, as she supported herself by keeping a boarding house. About this period, a tumour as large as a goose-egg made its appearance immediately above the right ilium, and was extremely painful. It confined her for the most part to her bed. On consultation with the late Dr. M'Clelland, it was agreed to open into the tumour. I accordingly plunged an abscess lancet into it ; a quantity of limpid serum immediately followed, but suddenly stopped. I enlarged the opening, and to my surprize found a large hydatid plugging it up. I perforated it with a probe and withdrew it. To this succeeded another and another. These hydatids were from the size of a common currant, to that of a goose-egg ; and one, from the size of a sac which was ruptured in the extraction of it, must have contained nearly a pint of water. I preserved some of them entire, at least an inch and a half in diameter. The incision continued open, and the patient to discharge these parasitical animals, for three or four months. At each discharge she obtained temporary relief from the dull pain which still continued. About this time the orifice closed, her strength and appetite failed, and a distressing cough supervened. I had no hopes that she could long survive. An occasional laxative, some expectorant medicines, and other palliatives, were all that she took. In about a fortnight the orifice opened again of itself, but no hydatids appeared ; a slight oozing of thin serum was all she discharged, but with considerable relief to all her other symptoms. It is now five years since the operation, and this alternate opening and closing of the orifice has continued, and still continues, with the same relief and aggravation of symptoms. She is, on the whole, more comfortable at present than she has been in any preceding year. On the lowest calculation, she must have evacuated from five to six hundred hydatids.

I know not that in any former case living hydatids have been seen in the human body. Yet did I at all times neglect accurately to examine their degree of vitality, which might easily have been done. This I regret extremely, as no such opportunity has occurred either to Morgagni or Baillie, both of whom have so minutely described them in the dead subject. They formerly have only been found alive in the inferior animals, particularly in the brain of sheep.

ART. IV. LETTER *on* FEBRILE CONTAGION: *addressed to* DAVID HOSACK, M. D. F. R. S. *Professor in the University of the State of New-York*, &c. BY JOHN W. FRANCIS, M. D. &c.

LONDON, *June* 16, 1816.*

DEAR SIR,

Within a few days I set out for Bristol, and in all probability this is the last letter you will receive from me dated London. The present communication might be devoted to many interesting subjects, medical and philosophical; but I am induced from several considerations to restrict my attention particularly to one.—You have long been acquainted with the important controversies that have existed on that grand subject of medical disputation, contagion; with the various and contradictory opinions that have been promulgated as to its nature and effects; and with the manner in which those controversies have been conducted, especially by American physicians. The question of contagion and infection has also occupied, as you well know, a large share of the attention of the medical writers of Great Britain, especially within the last few years. Much less diversity of sentiment, however, exists in this country than in our own, and in the discussion much less of asperity has been manifested. A single exception occurs in the case of Dr. Bancroft, in his late "Essay on the Disease called Yellow Fever:" a work intended by the author to prove that this form of fever is occasioned by the operation of marsh miasmata, and is non-contagious ; and of which performance it is due to the talents of the author to admit that he has displayed great learning and research. As one practically acquainted with the disease during its visitations in America for a period of more than twenty years, your own ample experience will have enabled you at once to perceive the fallacy of such speculations. Indeed, the volume of Dr. Bancroft has most materially aided in the establishment of the very doctrines which it was his object to overturn. Few writers have entered the field of controversy with stronger prejudices, and, perhaps, none have communicated their thoughts with less deference to authority and in more illiberal language. It would appear to be the opinion of Dr. Bancroft, judging from his conduct, that gross invective and personal abuse may supply the place of well authenticated fact and legimate deduction.

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* During the Writer's residence in Great Britain, finding the question, Whether the human constitution is susceptible of a second attack of Yellow Fever? occupying a large share of the attention of the medical profession, it occurred to him, that the ample experience enjoyed by the physicians of the United States, would go far in settling this controversy. With the view of calling the attention of American Practitioners to this subject, and of obtaining the results of their observations, the above letter is published. These results shall be made known in a future number of this Journal.

The revival in England of the controversy relative to the specific form and contagious nature of yellow fever, has been the means of giving birth to several works of great practical value, and, in my opinion, deciding the great question, if indeed any thing had been wanting after the laborious investigations of yourself and other American physicians. The volumes of Sir James Fellowes and Dr. Pym have just made their appearance here, and may not yet have reached you. The former author has published the results of his practice under the title of *Reports of the Pestilential Disorder of Andalusia, which appeared at Cadiz in the years* 1800, 1804, 1810, *and* 1813 *; with a detailed account of that fatal Epidemic as it prevailed at Gibraltar during the autumnal months of* 1804, *&c. ;* the latter, under the name of *Observations upon the Bulam Fever which has of late years prevailed in the West-Indies, on the coast of America, at Gibraltar, Cadiz, and other parts of Spain : with a collection of facts proving it to be a highly Contagious Disease.* As officers of high trust in the medical department of the Army, they have enjoyed unusual opportunities of observation: the manner in which they have drawn up the respective accounts of their labours is highly satisfactory, and the accuracy of each work is fully confirmed by official documents. Sir James Fellowes, as long ago as 1795, had numerous opportunities of witnessing the pestilential fever which committed such ravages among the British soldiery of St. Domingo, and he describes the Peninsular fever as appearing under a similar form of malignity, and showing many of the strongly marked characters of the St. Domingo fever. His history of the origin and progress of the disease clearly points out that there is a real foundation for the distinction between fevers arising directly from the miasmata of marshes and decomposed vegetable matter, and those that are the offspring of human effluvia or specific contagion. The account furnished by Dr. Bancroft under this head is clearly proved to be erroneous, and his statements, deficient as they are in the most essential requisites, will have little weight when compared with the judicious relation of the Spanish Professor, Arejula. In his observations on the disorder called the Walcheren fever, which prevailed so fatally among the troops of Zealand in 1809, and after their return to Great Britain, and which disease, notwithstanding the volume of Pringle,* has most absurdly been pronounced by some writers† to be the yellow fever, Sir James maintains that it possessed no contagious property, at least no evidence existed that the complaint ever had been propagated or communicated to those in attendance upon the sick. "This fact," says he, "was confirmed by my own experience, and by the testimony of all the medical officers of the army." "On the other hand," adds Sir James, "the numerous facts which have been recorded of the contagious nature of the pestilential fever of Spain are incontrovertible; they are detailed with simplicity and truth, and they must speak for themselves."*

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* Diseases of the Army.

† Vide the Medical Repository, &c.

Dr. Pym, in his Observations, has attempted to prove that the fever of Gibraltar was the same as the Bulam fever, so happily described by the learned and distinguished Chisholm; that it is a disease totally distinct from the bilious remittent fever of warm climates; that it has no connection with or relation to marsh miasmata; that it appears in the West-Indies only under peculiar circumstances; that it is contagious, and under a certain degree of temperature may be propagated from one country to another; that it attacks, in a comparatively mild form, natives of a warm climate, or Europeans whose constitutions have been assimilated to a warm climate; and that it differs from all other fevers, in having its contagious powers increased by heat, and destroyed by cold, or even by a free circulation of moderately *cool* air. According to Dr. Pym, under the name yellow fever, have been confounded three fevers, which he considers as totally distinct. The agency of marsh effluvia, I have already observed, he wholly rejects as a cause of the Bulam, Gibraltar, or real yellow fever. This you will perceive is striking at the root of that most pernicious error that has been so zealously and so widely propagated in our own country as well as in Europe, notwithstanding the evidence of facts to the contrary, and the happy nosological distinctions made long since by such authors as Blane, Chisholm, Lempriere, Clark of Dominica, Steward, Bard, Wistar, and numerous other practical observers of undoubted veracity. For I believe you will agree with me, that from the want of a due discrimination on the part of many writers who have professed to describe the fevers of the United States, as they have prevailed in our sea-port towns and in different inland places of our country, have proceeded most of the dissentions that have existed on this subject. This want of discrimination, too, may justly be considered adequate to the production of many of the histories of the diseases of America that have appeared, and in which we find fevers confounded arising from dissimilar causes, characterized by a different train of symptoms, and varying most essentially in the methods of treatment they require.* Dr. Pym's is no feeble attempt to counteract the influence which the unwarrantable opinions of Dr. Bancroft may have had in relation to this point.

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* Introduction, p. xxii.

But I have to solicit your attention to another important circumstance made known in the volumes of Dr. Pym and Sir James Fellowes, and to communicate which this letter has been written. Dr. Pym, who had the advantage of seeing the disease not only in Europe but in the West-Indies, contends, that the Bulam fever attacks the *human frame but once;* and supports this position by the strongest proof. I will not do injustice to his statements by attempting to abridge them. Irrefragible evidence is advanced by Dr. Pym, that the Gibraltar, West-India, or Bulam fever, (the malignant pestilential fever of Chisholm,) are the same disease. In a subsequent essay, Dr. Pym has enlarged on the subject of the disease affecting the human body but once.—I must be indulged in one or two extracts.

" At Gibraltar, during the prevalence of the disease in the years 1810, 1813, 1814, there was no well authenticated instance of a second attack : every person escaped it, who had

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* This language, I feel assured, will not be deemed too strong by those who impartially compare the different accounts of the fevers which prevail in the interior of our country, and more especially those that have made their appearance in the vicinity of lakes, with the histories that have been given of the pestilence or yellow fever as it has exhibited itself in New-York, Philadelphia, Boston, and other sea-ports and places within the United States. To consider the several kinds of fevers as grades only of one and the same disease, is a retrograde movement in medical philosophy.

had it at any former period : and this fact is now so well established there, that among the quarantine regulations against the introduction of the disease this year, (1815) *all the troops who have not passed it are encamped, while those who have passed it are doing the duty of the town.* At Cadiz, Carthagena, and Malaga, the fact of persons not being liable to a second attack of this disease, is considered to be as firmly established as it is in the small-pox."

"Two more proofs of the Bulam fever not attacking a second time, were in the 70th and 55th regiments. The first suffered severely from the disease in the West-Indies, in the year 1794, and returned to that climate from Europe in the year 1800, filled up with new officers, with the exception of six, viz. Col. Dunbar, Major Elliot, Captains Johnstone, Lawrence, Hutchinson, and Boat, who had had the fever at a former period in the West-Indies, and who now escaped it, although the corps buried ten of the newly appointed officers in a very short time."

"Upon a moderate computation, there were *one hundred and fifty* officers (civil and military) at Gibraltar, who had not had the disease before, and twenty-five who had passed it in the West-Indies ; and making an allowance for one or two doubtful cases, where the disease was so mild as not to confine the patient to the bed, one hundred and forty-five at least out of the one hundred and fifty were attacked by it, while every individual of the twenty-five who had it before escaped it ; proof positive that the Gibraltar, West-India, or Bulam fever, are the same disease, and that the human frame is not liable to be attacked by it a second time, even after a lapse of ten years." *Appendix to Dr. Pym's Observations.*

This same peculiarity marked the pestilential fever of Spain. According to Sir James Fellowes, it never has been known to attack the same person a second time in that country. "This fact," says Sir James, "which was first observed by the native practitioners, has now been confirmed by the experience of several years, and by the concurrent testimony of all the surviving inhabitants of those places, where the disorder had most prevailed." *Introduction, p.* xxiii.

I have dwelt so long on the performances of Sir James Fellowes and Dr. Pym, as almost to be deterred from referring to any other authority; yet I cannot forbear making a short extract from an account of the epidemic fever which occurred at Gibraltar, in the years 1804, 1810, and 1813, and for which the public are indebted principally to Dr. Gilpin, one of the inspectors of the hospitals. The paper throughout is one of singular merit, and eminently calculated to do away the doubts of the sceptical, and strengthen the faith of the wavering. It is gratifying to the philanthropist to read the answer given by Dr. Gilpin to the eighteenth query, addressed to him by the Medical Board of the army.

The points embraced in this query involved the consideration of several matters, to wit: were many of the attendants of the sick in private houses and hospitals attacked with the same disease, and where this did not happen, were there any circumstances evident that might have rendered such persons unsusceptible of the contagion; such as their age, previously having had a like disorder, particular precautions, &c.?

"In private houses, in most cases," replies Dr. Gilpin, "the attendants were attacked. There were undoubtedly many exceptions in the hospitals; but it was to be accounted for, as, generally speaking, the attendants were persons who had had the disease previously either in the West-Indies or in Spain, or here, in 1804. At the commencement of the disease last year, it was calculated that there were about five thousand persons within the walls who had previously passed through it; and after careful inquiry, there does not appear to be one well authenticated case of a person's having received the infection a second time. I heard, indeed, of three or four; but as the nature of the previous fever could not be exactly known, these exceptions have but little weight in so momentous a question. The exemption from a second attack, I am credibly informed, is firmly believed in Spain. At Cadiz, last year, though the fever put on the very worst symptoms, and destroyed the patient frequently in forty-eight hours, the deaths did not exceed, in a population of upwards of seventy thousand, fifty a day; and these were chiefly strangers. The Spaniards are so fully convinced they cannot receive the infection a second time, that having passed the disease is matter of great rejoicing among them: a medical certificate of the fact, is a sufficient passport into an infected town, which they enter without the smallest apprehension." Consult the Transactions of that active and distinguished association, the Medical and Chirurgical Society of London, vol. 5, for more ample details.

The immunity of the constitution from a second attack of yellow fever, is a peculiarity so strikingly characteristic of most disorders of an acknowledged specific nature, and of such great practical interest both in a social and political point of view, that it is extraordinary it should have met with so little notice before Professor Arejula made mention of it in the year 1806. "The yellow fever at Andalusia," says Arejula, (I avail myself of the translation of his account in *Sir James Fellowes' Reports, p.* 67.) "attacks persons but once in their lives, and it is of great importance to the physician to know this, in order to form his prognosis and his plan of cure, as well as for the individual who may have passed through this disorder, that both of them being assured of this fact, may step forward without fear to the relief of their fellow creatures who may hereafter be afflicted with so dreadful a malady." Dr. Pym, however, enjoys the reputation of being the first English physician who promulgated this principle. I have not the sources of information on hand to enable me to determine how many of the writers on the malignant fever, as it has prevailed in our country, have entertained this opinion, though I well recollect Dr. Lining to have been one; as may be seen in his account of the fever in Charleston, published more than sixty years ago in the Edinburgh Physical and Literary Essays, volume second. In the interesting correspondence on the yellow fever which was maintained a short time anterior to this period by Dr. John Mitchell, of Virginia, and Lieutenant Governor Colden, of New-York, nothing is alluded to from which we might infer their knowledge of this law of the disorder. See the American Medical and Philosophical Register, vol. 1st. and 4th. In the Facts and Observations of the College of Physicians of Philadelphia, on the nature and origin of the pestilential fever, after establishing the identity of the yellow fever which existed in that city in 1793, 1797 and 1798, with the West-India pestilence, the College state, that it is a circumstance that deserves particular attention, that "very few, if any, of the Creole French in this city, [Philadelphia,] suffered from the contagious malignant fever which prevailed here in 1793, 1797, and 1798, though the disease was introduced into their families; and children born in this country of Creole parents, died with it last autumn, while the parents and the children born in the West-Indies were entirely exempt from it." We look in vain, if my memory serves me, for any thing of the same sort in the Additional Facts and Observations a subsequent publication of the College of Philadelphia.

In the Sketch of the Malignant Contagious Feveras it appeared in the same city in 1793, Dr. Cathrall observes, "it does not appear to affect the same person twice. Although careful inquiry," adds he, "has been made by several of my medical friends and myself, it only appears that some of the patients had a slight relapse of fever, but without any of the distinguishing symptoms of the disease, and very soon recovered." It is much to be regretted, that the several histories of this disease published by that able medical annalist, the late Dr. Rush, should have been so confused and unsatisfactory on so momentous a matter. In his account of the bilious yellow fever of 1793, you will, nevertheless, find that the refugees from the French West-Indies "universally escaped the disorder," though this was not the case with the natives of France who had been settled in the city. On the other hand, Dr. Currie, of Philadelphia, in his treatise on the Synochus Icterodes, states, that several instances occurred of the disease affecting the same individual a second time, and under circumstances so unequivocal that it could not be *fairly* ascribed to a relapse. This assertion, you will see, is not strongly made, and may be deemed rather matter of opinion than matter of fact.

Dr. Currie also tells us, that the French West-Indians, particularly those from St. Domingo, almost to a man, escaped the disorder, though they made use of no precaution for the purpose, "while those from France were as liable to it as the Philadelphians." Nothing in relation to the security from a second attack of the disease is advanced by the late Professor Bayley, in his excellent volume on the Epidemic Fever of New-York, in 1795; though in the Collection of Papers published by Mr. Webster, this writer on the epidemic of New-York, of the same year, alleges that he knew not a decided instance of an individual labouring under a second seizure.———But at present I am not duly prepared to enlarge on this point, by reference to other American authorities.

Dr. Pym has referred me to a passage in Sauvages on this disease, in which it is asserted, that it operates upon the constitution but once. *Typhus icterodes* contagiosus est. Albos tantum, maxime peregrinos ex regionibus frigidis advenas, Indos, Hybridos, mulatros omnes, exceptis infantibus, una tantum vice afficit: nigri vero ab eo morbo nonquam afficiuntur." See Nosolgia Methodica, tom. 1. p. 316, of the quarto edition of 1768. We must here bear in recollection, that Sauvages has depended mainly on Lining, as authority for this peculiarity. Does your own extensive experience in the malignant epidemic of New-York, agree with the opinion that the human constitution is invulnerable to a second attack of yellow fever, corresponding in this respect with small pox, and other specific disorders? In answer to this question, which has frequently been put to me by practitioners of medicine in England, I have uniformly ventured to assert that it holds good as a general fact. Those who have once had the disease are certainly *less susceptible* of its influence a second time.

Permit me now to make known to you the important results of the recent deliberations of two of the most distinguished medical associations of this kingdom. The decisions of the Royal College of Physicians of London and of the Army Medical Board are at length brought to a close. These two learned bodies, alike distinguished for scientific attainment and practical knowledge, have been for a considerable time past devoted to a consideration of all the facts connected with the nature and character of the yellow fever, particularly as it has of late years appeared in Spain. The Royal College have pronounced that the yellow fever is a highly contagious disease, which decision they have reported to the Lords of the Privy Council. With respect to its attacking the human frame but once, they say they think it *extremely probable*, but that upon a point of such importance they cannot venture to give a decided opinion. The Army Medical Board, at the head of which presides Sir James M'Gregor, have also given it as their opinion, that the yellow fever is in its nature contagious; and they further add their conviction, that the fever of Spain is not only strictly contagious, but like other disorders of a specific character, it affects the human frame but once. I have been kindly favoured with an abstract of these proceedings, and I herewith enclose an extract from the official report on Dr. Pym's publications made by the Army Medical Board. The operation of climate, soil, and other local causes, in adding virulence to febrile contagion, may be considered almost an axiom in physics; and the necessity of a strict adherence to your improved system of quarantine laws, and all municipal regulations for the purpose of domestic cleanliness, cannot be too strongly enforced. On this subject the Royal College and the Army Medical Board are united in opinion.

(COPY.)

EXTRACT FROM THE REPORT UPON DR. PYM'S PUBLICATION BY THE ARMY MEDICAL BOARD.

Army Medical Board Office, 6*th May*, 1816.

"It is due to Dr. Pym to state, that we consider him to have been the first English medical man who promulgated the opinion, that the disease in question (the Bulam fever) is capable of attacking the human frame but once; and if that opinion be correct, which we believe it to be, it is certainly an important fact, and led Dr. Pym to employ those persons as attendants on the sick, who had undergone the disease, and therefore were not likely to be affected by the contagion of it, and thus probably saved many lives. Under these impressions, we beg leave to recommend the industry and research displayed by Dr. Pym, in his book, to Lord Palmerstone's favourable consideration.

"Signed,

"J. M'GREGOR,
"W. FRANKLIN,
"W. SOMERVILLE."

The advocates for the unity of disease will, I believe, find it insuperably difficult to reconcile with their theory, the facts which I have thus hastily communicated to you; while the fundamental principle, that there is a radical difference between remitting fever and yellow fever, between fevers depending upon marsh miasms as their source, and those that take their rise from human contagion; in short, that yellow fever is a distinct idiopathic disease, acquires additional support. It may not therefore be of disservice to make known the purport of this letter. The doctrine maintaining that different fevers are of one common origin, is in reality so unfounded in fact and so pernicious in its consequences, that the sooner it is discarded, the better it will be for the interest of science and humanity.

Before I conclude, permit me to add a few lines on a subject not wholly foreign to the object of this letter, the *plague.* The account of the origin and progress of the plague in the island of Malta, in the year 1813, drawn up by Dr. Calvert, physician to the forces, and printed in the 6th volume of the Transactions of the Medical and Chirurgical Society of London, is a document of great value. The reasoning of the author, deduced from the evidence which a faithful narrative affords, seems to be very satisfactory.

Contact, he maintains, is the most certain mode of communicating the disease, but he is inclined to deny that it is essential to the propagation of the contagion.

"It appears to me," says Dr. Calvert, "that this contagion or principle of plague, is diffusible in the atmosphere to a distance greater or less from an infected body, according to the climate and season of the year, and possibly to other peculiar states of the atmosphere, with which we are unacquainted; that in the spring or summer season a single infected person is sufficient to contaminate the air of a whole city; and that those who happen then to be exposed to febrile causes, or otherwise predisposed, are the first to become its victims. That these newly infected persons generate a fresh supply of poison, increasing its strength and influence, till at length it becomes so ~~powerful~~, that nothing but the winter season will entirely put a stop to it."

The various reports that have been so industriously circulated concerning the contagiousness and non-contagiousness of the plague, especially as it prevailed in the army of the East, and the unfair and contradictory statements that have been made relative to Baron Desgenettes, induced me, while in Paris in the spring of 1816, to seek an interview with that gentleman, in order to ascertain the truth on this interesting subject. Dr. Delile, the companion of Dr. Desgenettes as a member of the Institute of Egypt, accompanied me. What practical advantage may arise from inoculation for the plague we are not yet able to state; that the experiment is not without great danger is sufficiently well ascertained. The Baron distinctly declared that it had ever been his settled opinion that the plague was a contagious disorder ; that his extensive experience as an officer of the medical staff, had only served to confirm him in that opinion ; and, further, expressed much surprise, that any account should have been made public representing his views in a different light. The Baron inoculated himself with the matter of plague, though he felt persuaded that the disease was of a specific character, and had almost hourly evidence of its contagious effects : but more fortunate than the incredulous Whyte, he did not fall a victim to the experiment.

I am aware how confidently and unblushingly the case of Dr. Whyte had been denied. So far, however, from any doubt being entertained by the medical philosophers here, as to the accuracy of the statement of his inoculating himself with the matter of plague, it is well known that his preconceived notions of the nature of that pestilence, were the cause of his rashness and premature death. Of this I have been assured by personal communication with Sir Gilbert Blane and Sir James M'Gregor.

When I took pen in hand, I did not expect to produce so long and tedious a letter. My apology must be the nature of the subject, interesting beyond all others to an American physician.

With due respect, I remain, dear Sir,

Your friend,

JOHN W. FRANCIS.

Dr. David Hosack, New-York.

ART. V. REMARKS *on the* DISEASES *of the* FEMALES *of several* INDIAN TRIBES *west of the Mississippi: in a letter to* JOHN W. FRANCIS, M. D. *Professor of Midwifery.* By JOHN D. HUNTER.

DEAR SIR,

I now send you, agreeably to promise, my observations, which you will receive with all due allowance. They are gathered during a residence of many years among the people whose manners I have attempted to describe. It is well known that there is much difference between the diseases of savage and civilized life. Perhaps you will perceive, from what I now write, a difference no less striking in the respective methods of cure. At all events, I indulge the hope that I may have advanced something in this communication, which may interest you as professor of obstetrics.

I shall, by way of introduction to the present subject, offer a brief notice of the habits peculiar to the females of savage life, and the influence those habits exercise on their health.

The domestic labour of Indian life devolves almost entirely on the female. But the custom, in this respect, differs essentially among the various tribes, and is accommodated to the various circumstances in which they are placed. Those tribes, for instance, who are well settled, and live most distant from the encroachments of their enemies, have most domestic drudgery to perform, yet are, in a measure, free from much of that labour and fatigue which the more roving tribes are subjected to. The former tribes are better calculated to increase in numbers, both from their more certain supplies of food, &c. and from their greater security from their enemies. Those of the latter class have little or no anxiety about the wants of to-morrow, and are never long enough in a particular place to become tired of it, but enjoy an almost continued novelty from constantly roving from one region to another.

There might, at first view, seem to be a decided balance in favour of that situation wherein the supply of food is more certain, and where the inhabitant is enabled to acquire those conveniences from which the other is entirely precluded. This, however, is true only to a limited extent; the former are exposed to many wants in their more settled life, which nothing but civilization and improvement in the mechanic and liberal arts can supply: they suffer much from the varied haunts of the game, and are compelled to leave their homes several months in the year, during which time their women and children are frequently carried off captive by their enemies. No persons, however, enjoy more freedom from disease than those who are thus situated; and their mere ordinary employments, while they supply the means of subsistence, tend greatly to promote and improve the health.

MENSTRUATION.

I shall now speak of that great change in the condition of the female constitution, which is ever regarded, by the Indians, with superstitious horror in the one sex, and of such extreme moment by the other. This, however, is not to be wondered at, when we advert to the great pain, and even danger, which often attend any interruption of this salutary function. They can, for the most part, readily foresee at what time the menstrual period is about approaching; and, accordingly, make the requisite preparations for the same. The female thus circumstanced, repairs to a house or lodge at some distance from the wigwam, and continues there during its flow. I believe this is always accompanied with more or less of pain; it makes a great change in the life of the girl, and they now say she must be done with children's things, and be a woman. This is now the age at which they are considered suitable companions to go in company with the squaws, to get wood, drive up the horses, to pack in the meat killed by the hunters, to cultivate the corn, beans, pumpkins, &c. dress robes, make mockasins and leggings for the men, while previously their principal business was roving about collecting shells, flowers, &c.

They seldom exhibit evidences of puberty before the age of sixteen years, which gradually announces the approach of the catamenia; this state prepares them for child-bearing. They usually suffer some change of complexion, an increase in the size of the breasts, and shooting pains through various parts of the body and limbs. Menstruation scarcely ever happens earlier than at the above period. While suffering from menstruation, they never associate with the men, nor prepare any thing for their accommodation; though, in this respect, some are more scrupulous than others.

The first time an Indian woman becomes pregnant, which is seldom under the age of twenty, she is commonly somewhat peevish and fretful, and frequently indulges in strange whims and fancies. I have never known more than one or two deformed infants among them; and never an instance of marks being on different parts of the body. Giddiness or vertigo is one of their troublesome symptoms during pregnancy; pains of the head, back, and limbs, also occur. Another symptom often very troublesome, is acidity, heartburn, or other derangement of the stomach. Much more, I doubt not, might be said on this head.

On the approach of delivery, she goes to the cabin prepared for the accommodation of the women, in company with her best friend or her mother, and very seldom with more; sometimes in a few hours she is relieved of her burden; sometimes labour is more lingering, particularly in case of a first child.

The Indian females have an advantage over those of civilized society, in having easier and shorter labours, and in being able to return to their ordinary pursuits much sooner.

I do not know, however, that they have less pain than the healthy and robust white females who live simple and temperate lives on the frontier settlements. Though I have known cases of deformity more frequent among the frontier whites than among the neighbouring Indians. I have known frequent instances of doublets, and one of triplets, among the whites in the back settlements, but never but one or two cases of twins among the Indians, and never a case of three at one birth. They make use of unctions of bear's oil, or of fat of the buffaloe, to anoint with.

The lodge prepared for the accommodation of the parturient Indian is provided with a mat laid on a kind of frame made of forked posts driven in the floor, and poles laid on the forks. On this robes of buffaloe skin, or blankets, are placed for her accommodation. Her companion performs the duty of accoucheur. After delivery, the child is taken proper care of by the friend of the patient, who, having cut the cord, washes the infant abundantly with cold water, imbued with sassafras, &c.

They always procure a nurse whose child is near the age of the infant just born, whom they employ for two or three days until the mother is competent. When the mother has recovered herself, all her intimate friends go to wish her well; she then returns to her family, though, for the most part, not without having bathed herself.—They seldom die of childbirth; the cases I have mentioned are what I call natural births. But there are instances where they linger for a day or more; and sometimes they suffer the most excruciating pains and tortures, during which the old women administer their bitters, teas of dittany, &c. and, I believe, frequently do much good by their remedies.

There is seldom or never any need for the doctors to interfere in these cases; though this is different among different tribes, as among the Chippeways and Bicaras. I have known the Indians to make use of many foolish charms to prevent disease, which they believe to be inflicted through the agency of the evil spirit; whom they think to be very much intimidated by the eel's liver: this I have known to be given in these, as well as in other instances. If the lochial discharge continues too free, or too long after the milk begins to flow, they dig up the placenta, or after birth, and bury it in another place.

OF THEIR NURSING THEIR CHILDREN.

The Indians bestow little attention on their children, more than to supply them with sufficient nourishment for their health and comfort, and to protect and preserve them from injury, and the vicissitudes of the weather. They seldom, I may almost say never, give them animal food until they have teeth sufficient to masticate it well. During the sugar-making season, they give them sugar and syrup, which well agrees with them; they keep their children to the breasts from eighteen months to two years. Their children do not cut their teeth until they are a year old, and they do not suffer as much as white children during dentition. Their food being more simple, renders them, as I suppose, less irritable during their state of infancy. They seldom or ever eject their food in a curdled state. Their accommodations as to nursing are extremely simple. The child is confined to a piece of bark of a large tree, or board; a soft piece of buffaloe or bear's skin, is laid on the board for the infant to lie on. The child is confined by two bands, made, most commonly, of the long, soft hair of the buffaloe. Here, by the side of a tree, or in the wigwam, the infant is kept for hours together, while the parent may be gone for miles in quest of wood or fruit. The mother will sit with the utmost composure and hear her child cry without attempting to pacify it, when she is certain her offspring is well. They say that crying is good for the child; " it will make him grow," and he will stop when he gets tired. It must not be inferred from this, that the Indians are indifferent to the comforts of their offspring, or that they are destitute of parental affection. The extreme anxiety and solicitude which they manifest when their children become sick, or are accidentally injured, proves the reverse beyond a doubt. Let their families be in danger from an enemy, or exposed to fire, &c. and I venture to say the strength of the parent's affection is shown in proportion to the danger which threatens the safety of the offspring.

DISEASES OF LACTESCENT WOMEN.

In long continued journies, the women frequently become enfeebled from suckling their children, as also from fatigue, hunger, exposure, and other causes. This brings on a discharge of a white appearance from the genital parts ; and the men will at such time have no connection with them. This discharge frequently continues a great length of time, and if it does not yield to remedies, great loss of strength, night sweats, and wasting away, follow, and they are reduced to the appearance of a skeleton. If they wean the child, they often get better, and sometimes recover ; but after this they seldom or ever again have children. They are in fact less prolific than white women, and seldom have more than four or five children ; and the interval of births are for the most part, three or four years. To please the child, they peal the bark from the body of a suitable tree, while the sap is up, and split it in halves, and lay the infant in it when it is restless. This enables it to be rocked, and thus affords the accommodation of a cradle.

I shall now make a few remarks on their method of treating diseases peculiar to females and infants, that is, children at the breasts.

DIFFICULT OR PAINFUL MENSTRUATION.

This is treated by puking, strong harsh purging, and stimulating roots and herbs. One of their favorite drugs of this character, is a strong tea made of dittany, mountain tea, and wild ginger. Of this drink they take very freely three or four times a day. Where there is much weakness, which is sometimes the case, accompanied with loss of appetite or palpitation of the heart, they give the puccoon root (sanguinaria Canadensis) in very small quantities, united with slippery elm tea, (ulmus Americana;) I cannot say with what success, though I believe they place much confidence in its emmenagogue properties. They use liberally sudorifics in this complaint, and carry them at times to an injurious extent. They expect most benefit from their remedies when they apply them about the time they suppose the discharge is about to commence, or a few days previous. They heat large stones, lay bitter herbs on them, then pour water on them, and wrap them in a blanket or buffaloe robe. They are so placed that the steam from the rocks and herbs may come in immediate contact with the seat of the disease. They place great confidence in these means as a method of cure. They also use the colt's foot and deer's tongue in this complaint.

FLUOR ALBUS OR WHITES.

This is a disease which I have often heard mentioned as existing among the degraded Indians who live near the frontier settlements, who are addicted to habits of intemperance, and also among such as inhabit wet, marshy, sickly places, and have become emaciated.

They have many remedies for this complaint: they bathe the whole body, and make liberal use of local washing. They make great use of the bark of the sarvas tree, and sour wood; they use a strong infusion of the red root, with the rust of iron, or something similar put in it, which they obtain from the cliffs of rocks on the banks of the rivers, and salt licks. This wash has a great power in stopping the discharge for a while, but they say it will return again.

When the patient labouring under this disease is suckling a child, she facilitates the cure by weaning the infant sooner than she otherwise would have done. They also use the puccoon in this complaint in combination with sweet gum.

I know nothing of its value in such cases, or how much it merits their confidence. Snake skins are sometimes worn girdled round the waist, to relieve them. This is also a remedy for painful menstruation. The Indian female considers the whites as one of the most troublesome disorders, though not in itself dangerous, and they omit no means in order to get rid of it as soon as practicable: their husbands have no connexion with them while they are in this condition.

FLOODING.

This is a disorder which sometimes happens among the female Indians, and from other causes independent of those connected with parturition or abortion. I have witnessed, after long and fatiguing journies, or from heavy burdens, this affection to come on when the female has been compelled to desist from travelling or other business in which she may have been engaged. Their remedies in these cases are not few in number. Some are the same as those used for the preceding discharge I just noticed. But their chief remedy is cold, and when they succeed in arresting the hæmorrhage, if the patient be much prostrated, they give her strengthening articles. While the discharge continues, they keep the patient lying on her back, and frequently wrap a buffaloe robe or something of the kind under the small of the back, so as to raise those parts above the level of her head. One of the means of cure is to take a large bunch of fine moss or wool, dipt in very cold water, and apply this immediately to the bleeding parts. They also make use of ice. They apply cold externally by means of skins or pieces of blanket wet in very cold water placed over the patient's abdomen. This often answers their best wishes.

DISEASES OF CHILDREN.

The diseases of the Indian children are few, compared with those of civilized life. *Irritations of the bowels* sometimes occur, yet they are not at all frequent; and they have immediate means for relief in a prescription made of burnt muscle shells, powdered and given in dittany tea. *Diarrhœa* is another of their infantile disorders; they have many ways of curing it. The before mentioned means for removing the irritations of the bowels, is one of them: gentle sweating and purging are also thought much of by them for this purpose.

Diarrhœa, or an extreme looseness of the bowels, is an affection which exists, particularly in the summer months, when there is an abundance of acid fruit, and which they greatly indulge in before it is ripe. They think that purging will carry off the complaint, and accordingly use means for this end. Sweating they also consider among their remedial measures. Sometimes they plunge the patient in the river, which I have thought has now and then been of great service.

CROUP.

This is one of the most distressing complaints which occurs among the Indian children: it often proves fatal, and that in a very few hours. The disorder comes on for the most part suddenly, and after cold and damp weather. They look with alarm upon its first appearance, and it is not without very just reason that they do so; for no disease to which their infants are subject proves a greater source of inquietude. The little patient gets its breath with great difficulty and labour, and seems to be tortured with continued efforts to retain it; and this, besides, produces a rattling sound or kind of wheezing which may be distinctly heard for several yards from the sufferer.

They almost uniformly have recourse to emetics; they puke the child as the first thing to be done, as this is accomplished by the Indian physic, (sanguinaria Canadensis) or crow's foot, or yellow root, or podophyllum peltatum. They next immerse him in the vapour arising from hot stones, which they confine to the body of the child, by spreading a blanket or buffaloe robe over him when they are not within a convenient distance from the sweat house ; they also give pretty warm sweating teas. I have known them to excite vomiting by running a soft feather down the throat of the patient, for the purpose, as they say, of bringing away the phlegm, which they think makes him breathless, or to breathe hard. When they die of this complaint, they usually go off convulsed, with very distorted features, and much apparent agony. Their mothers support them in an upright position, so that the little child thus affected may breathe in the most favourable condition. Their death is a hard struggle, and they seem to die exhausted. I do not think, from all the information I can obtain, that your practitioners among the whites have a greater dependence on emetics in this singular disease than the Indians have.

WORMS.

I question whether the Indian children are as much troubled with worms as the more puny children of the whites. I believe the more sickly the Indian children are, the more they are likely to be troubled with this complaint. This is an evil, however, which seldom proves serious : the Indians are generally successful in destroying them ; and for this purpose they give active purges, and teas of different kinds of roots and herbs ; as the star root in the form of tea, yellow root, and the magnolia : the puccoon is also at times deemed a good remedy.

The seeds of wild cherries are pounded and put in warm water : this is given to children as a puke, and also as a purgative.

I shall bring this communication to a close ; having extended it to a greater length than I originally intended. Several topics I might perhaps have more fully and satisfactorily treated, particularly the subject of menstruation, and the period of gestation. You will overlook the inaccuracies of my style, and place, with perfect fullness, reliance on the facts I have stated. I have seldom indulged in an observation, the accuracy of which I was not perfectly aware of. The Indians are commonly very reluctant in communicating a knowledge of their remedies to any strangers, unless they have the greatest confidence in them, except to the members of their own particular tribes. They consider the healing art a great secret revealed to them by the *Good Spirit*, who visits them during their sleep. These supposed revelations, however, I consider as nothing more than ordinary dreams, but which they always look upon as ominous. It is somewhat strange that the impressions made during their sleeping hours, should enable them to find with so much accuracy their wished for objects, whether it be medicine for their maladies, or game for their support, or a spring of good water when they have been travelling a great way, and know not where to procure a supply.

It is, I repeat, seemingly strange to the hunters and traders who go among the Indians, and are induced to believe that they actually have some mysterious intercourse with supernatural beings. It proceeds, however, from a want of more minute attention to their habits and manners; when these are understood, the charm ceases, and the sleeping impression, which always or generally is the picture of some prairie or fine grove abounding in game, or some pure stream of water, &c. being awakened in the mind by previous information, he pursues the wished for good with unabated ardour until he finally succeeds: while on the other hand, the trader, less accustomed to this mode of life, to the courses of the waters, the haunts of game, &c. becomes discouraged, and ascribes the superior success of the Indians to some superior knowledge of events out of sight. Thus they wish to make the ignorant whites, who occasionally visit them, to form an exaggerated estimate of their knowledge; so it is with their remedies; yet it may be said, that the Indians very generally place great reliance in dreams.

Accept, Dear Sir, what I have thus written; and if it shall contain any thing novel or interesting to you, I shall number those among my well spent hours which I devoted to writing it.

Your friend,

JOHN D. HUNTER.

DR. J. W. FRANCIS.

ART. VI. FACTS *relative to a* DISEASE *generally known by the name of* SICK STOMACH *or* MILK SICKNESS. *Communicated in a letter to* JOHN B. BECK, M. D. By Dr. LEWIS C. BECK.

THE last number of that valuable work, The Philadelphia Journal of the Medical and Physical Sciences, contains three communications, two of which are extracted from the Western Quarterly Reporter, on the subject of a singular disease, commonly known in the Western States by the name of Sick Stomach, Milk Sickness, &c.

The following facts, collected during my residence in that part of our country, may throw additional light on this subject, and are submitted entirely to your disposal.

The disease prevails in the vicinity of St. Louis, and St. Charles, in the state of Missouri; and Edwardsville, in Illinois. It is confined to the low lands about the confluence of the Missouri and Mississippi Rivers, and has not hitherto been observed on the "bluffs," or highlands. These lands are in many places heavily timbered, and are subject to annual inundation.

The first appearance of this disease in cattle, is observed by their noses being parched and turned out. If while in this situation they are driven, they frequently fall down, and either expire instantly, or remain for some time unable to move, and apparently in great pain. Should they survive the first attack, they are shortly after seized with violent nervous affections, accompanied with a weakness of the limbs, and a complete prostration of strength. These symptoms, however, rarely appear, except after the animal has been heated by exercise. Horses frequently fall dead in the middle of a prairie, leaving the traveller to make the best of his way on foot; and cows have been known to expire in consequence of having been driven from the pasture to the barn yard.

The disease is not always fatal; for some animals have been afflicted with it during several seasons, and have finally recovered. Others are affected so slightly, that its symptoms only appear after the most violent excitement.

A few cases have occurred in the human subject, although they are much rarer about St. Louis and St. Charles, than in other parts of the country. Excessive vomiting is a prominent symptom; in other respects the disease assumes the form above described.

In that section of country, the cause of the disease is generally supposed to be a poisonous plant. Those who eat the flesh or milk of cattle thus affected, are said to take the disease. And whether this be true or not, it is so firmly believed in St. Louis, that the trustees of the town, during the last summer, cautioned the inhabitants from purchasing butter made in the infected district. This disease has also been observed in many parts of the state of Indiana; near French Lick, on the main road from Louisville to Vincennes, it prevails to an alarming extent. It is worthy of observation, that while the lands on the Missouri and Mississippi above referred to, are low, rich, occasionally covered with a thick growth of timber, and annually subject to inundation, they are here elevated, hilly, and frequently consist of barren "knobs," entirely destitute of vegetation. In fact, the character of the soil or surface in these two situations is diametrically opposite. In the former also, the inhabitants are subject to intermittent and bilious fevers, whereas in the latter they are uniformly healthy.

In the district of country last mentioned the disease is called the "*Slows*;" a name by no means inappropriate, as those who are afflicted with it are obliged to be very slow and cautious in their movements, in order to prevent a recurrence of its painful symptoms. The disease at first appeared in horses, cows, hogs, &c. Dogs are also very subject to it; so much so, indeed, that in the whole neighbourhood at French Lick, there is scarcely a single one free from it. Frequently while following their masters, particularly if the weather is warm, they fall down apparently lifeless, in which state they continue for a few moments, and then resume their journey.

In every case where animals who have died of this disease have been dissected, the heart and aorta have been found greatly enlarged, and their inner coats sloughy.

In the human subject, the symptoms are similar to those in brute animals. Persons who are affected with the disease, if they wish to remain comfortable, are obliged to keep themselves perfectly quiet. Upon the least exertion, they suddenly lose all muscular power, their limbs are unable to support their bodies, and they sink down in a state of complete exhaustion and stupor. They soon, however, recover from this state, and remain comfortable until the next excitement.

Dr. Austin, an intelligent physician residing in the vicinity of the 'Lick,' informed me of the case of a lady whom he had seen during the last winter, who died of this disease. She had been affected in the manner above described for upwards of two years. For several days previous to her death, the unhappy victim was unable to move without the greatest pain; her respiration became gradually more and more difficult and painful, until at length suffocation put an end to her sufferings.

The same gentleman related the case of a young man, who was during the last winter suddenly attacked with the disease, while engaged in hunting; he fell down in a state of insensibility, and as the weather was cold, must have perished, had it not been for a fire that had accidentally been kindled in the woods.

In these two cases the disease was not attended with the excessive vomiting, and the nervous affections, which are usually observed. Those symptoms, however, have occasionally appeared, but are by no means alarming.

I shall now briefly notice the causes to which this extraordinary disease have been ascribed.

A Poisonous Plant.—In the neighbourhood where the sick stomach has prevailed, it has generally been ascribed by the inhabitants to the above cause ; and they have been confirmed in this opinion by the observations of some intelligent physicians. Although it is not my present intention to enter into a discussion on this subject, I shall mention a few facts which appear opposed to this hypothesis. 1st. On the Mississippi, the plant which is said to produce the disease is a species of Cicuta, (cicuta maculata, L.) growing in the low and wet grounds. I have not ascertained whether this plant is found at French Lick, but from the appearance of the country, it is probable, at all events, that it is not very abundant. 2d. By referring to botanical authors, we find that this plant is common in wet meadows and ditches, from New-England to Carolina, and still no cases of this disease have occurred to my knowledge east of Ohio. 3d. At French Lick, dogs were affected with the disease, previous to the death of any cattle whose flesh they could have eaten.

A Poisonous Mineral.—In the mine counties of Missouri, a disease prevails among cattle, called the " Mine Sickness." Mr. Schoolcraft, in his Views of the Lead Mines, observes, " Cows and horses frequently die without any apparent cause. Cats and dogs are taken with violent fits, which never fail in a short time to kill them. This has been accounted for by supposing that they inhale the sulphur which is so abundantly driven off in smelting lead, and cattle are often seen licking about old furnaces. It is more probable that it arises from the sulphate of barytes, which accompanies the lead ore. This consists of sulphuric acid united to barytes—*a poison to animals.* The mine sickness is wholly confined to quadrupeds."

It is supposed by many, that the disease here mentioned is the same which prevails near St. Louis, and that it is there caused by some mineral substance mistaken by cattle for salt.

In favour of this they adduce the fact, that no cases have occurred, which could not be satisfactorily traced to a circumscribed district about the mouth of the Missouri.

A peculiar state of the Air.—This is generally considered as the cause of the "*slows*," by the physicians and inhabitants at French Lick. It is observed that the disease is not produced in the human subject by eating the flesh or milk of animals affected with it, for it appeared in the former about the same time that it appeared in the latter. From this it is inferred, that it is originally produced in both by the same cause. Dr. Austin informed me, that while the cows in the neighbourhood were affected with it, their milk, and the butter made of it, were used, without producing any ill effects. The family of the female whose case is mentioned above was numerous, and had the disease been caused by improper food, it is not probable that she would have been alone affected.

It should be remarked, that an opinion is also prevalent, that if cattle are kept from going out previous to the falling of the dew, they are never affected with the disease.

I shall conclude this desultory epistle, with the request that you will at some future time favour me with your opinion as to the cause and treatment of this singular disease.

Yours.

L. C. B.

ALBANY. August, 1822.

ART. VII. FACTS *on* VACCINATION. BY JAMES M. PENDLETON, M. D. *and* J. SMYTH ROGERS, M. D.

To the Editors of the New-York Medical and Physical Journal.

GENTLEMEN,

If you think the following results, drawn from the practice of about five hundred vaccinated patients, present any circumstances worthy of record, you will confer an obligation upon us by giving them a place in your Journal. They present some peculiarities, which, although not calculated to throw any additional light on the anti-variolous power of the Cow Pox, may yet furnish some points of interest to medical readers.

Dr. Jenner, in the course of his laborious investigation of this disease, has drawn some general conclusions from his practice, which do not agree altogether with the facts which we here noticed. He states, in the first place, that in Cow Pox, the eruption never occurs but in the place where the virus is applied: we have had one case which furnishes an exception to this rule.

Mary Gallagher, aged 8 years, residing at No. 99 Crosby-street, was inoculated on the 2d of March; the vesicle proceeded through its regular stages, and toward the period of its decline, two more appeared on the back; these were succeeded by two more around the elbow, and went through their regular stages; the efficacy of this vaccination was tested by a re-inoculation. This was the only case presenting this peculiarity which we had. In a conversation with Dr. Post, he informed us that he had met with one case of a similar nature, where vesicles going through their different stages appeared on different parts of the body. Dr. Jenner observes, secondly, that the Cow Pox, under the most unfavourable circumstances, never proves fatal: the following case will show that a fatal termination does some times take place.

Harriet Ann Ross, aged 6 weeks, at No. 40 Augustus-street, was vaccinated on the 7th of May; about the period at which the constitutional symptoms showed themselves, an erysipelatous inflammation appeared around the vesicle, extending over the arm, and involving the head and chest; it continued to increase, until it included the lower extremities, when the child sunk under the irritation it produced. From an attentive consideration of the circumstances of the case, and the appearance of the vesicle, we were led to conclude, that its unfortunate termination was to be ascribed to a peculiar irritability of constitution, rather than to any morbid condition of the virus. Woodville, in the course of vaccination of an equal number of patients, mentions one case having proved fatal, but does not mention the manner in which death was produced.

The following varieties in the appearance and state of the vesicle we have noticed, and found not to affect the success of the vaccination. 1st. The vesicle will anticipate its regular period, and go through its different stages in a shorter time than usual. Case 1st. John Conroy, aged 9 years, 95 Mulberry-street, was vaccinated Jan. 13th, 1821; on the third day the vesicle was completely formed, and filled with transparent lymph; a number of patients were inoculated from this matter, in whom the different stages of the disease preserved their ordinary periods. Case 2d. John Herbert, aged 40, residing at No. 15 Moore-street, was vaccinated with three of his children, May 6th; calling on the 8th day to examine them, the vesicle on the father's arm had the dark mahogany coloured scale, that is usually formed on the 14th day, whilst those of the children presented the usual appearance on the 8th day; they were all inoculated at the same time, and with matter from the same arm; the father stated, that on the second day, his arm was similar to that of the children on the day we visited them. These were the only cases in which the vesicles anticipated their regular periods to so great a degree, but numerous instances occurred where they formed on the 6th and 7th days. Instances where the appearance of the vesicle was postponed beyond the usual time have frequently presented themselves ; in these, the first evidence of its effect has been visible not until the 8th, 9th, 10th, and even 14th day, from which time it has gone through its regular stages : under these circumstances, a second inoculation has been performed, where the first was thought to have failed ; and as the constitutional symptoms developed themselves, the place first inoculated has taken on the disease, and a regular vesicle formed, going through the periods simultaneously with the other.

Irregular Vesicles, or where the vaccination cannot be depended on. In these cases suppuration takes place on the 3d or 4th day, the scale formed is of a soft texture, resembling that of some forms of Porrigo, and totally distinct from the dark mahogany coloured scale of genuine Cow Pox. It wants, also, the central depression and elevated edges of the true vesicle ; the inflammation of the areola is more defaced, and of a darker colour.

It has been supposed that the virus, after being kept a certain length of time, loses its power of communicating the infection, and undergoes a species of putrefaction, by which it generates a spurious disease. That this is not the case of the scale, we had an opportunity of proving. In consequence of the failure of matter in the summer season, we were compelled to use a scale which had been taken 6 years before ; 9 persons were inoculated, and one took the disease ; the vesicle was regular, and furnished matter for inoculating many others.

It is generally recommended not to take lymph from a vesicle after the areola has formed around it ; we have not found that this circumstance has produced any effect on the vaccination, provided care be taken, that the lymph is perfectly transparent : this is the only essential circumstance.

In introducing the lymph, we make use of scarifications, and draw as little blood as possible. This method we have found preferable to puncture ; and it may be a question, whether the less frequent occurrence of inflamed vesicles now than formerly, may not be ascribed in some measure to the substitution of *scarification* for *puncture*.

ART. VIII. TWO CASES *of* NEURALGIA, *treated by the Carbonate of Iron: communicated to the Editors of the Journal, by* DR. J. EIGHTS, *of Albany.*

To the Editors.

GENTLEMEN,

WHEN a remedy is introduced for a disease which has often baffled the skill of eminent men in the profession, I conceive it to be the duty of every physician, who has an opportunity of testing its effects, to make known the result.

Under these impressions, and without any remarks on the nature of the disease, or the modus operandi of the remedy, I enclose you two cases of Neuralgia which have occurred in my practice, and in which the efficacy of the carbonate of iron in relieving that complaint is evident. Should you think them of any practical utility, they are at your disposal.

In the month of March, 1821, Mr. Van A—n, of the town of Guelderland, brought his son to me for advice. He was 18 years of age, tall, but of a feeble appearance. The account which his father gave was, that about a year previous to this time, he was troubled with severe pain in the right side of his face, extending to the upper lip, and to the eye of the affected side, which always produced a plentiful flow of tears. The duration of the pain, at first, was short, and it occurred at long intervals, a week often intervening between them. As the complaint advanced, the paroxysms were more violent, of longer continuance, and the intervals much shorter; and at the time I saw him, he had two or three attacks daily. Several of his teeth had been extracted without any relief; his gums had been largely scarified, blisters had been applied, emetics and cathartics had been repeatedly administered, and opium taken internally and applied externally, but all with little effect. The only relief he obtained, and that but partial, was from the spirits of turpentine rubbed along the gum and side of the face. After using the remedies generally advised for this distressing complaint, I determined to try the carbonate of iron, of which I had just then read an account, as used and recommended by Dr. Hutchinson of London. After evacuating the stomach and bowels, half a drachm was given three times a day: on the day that he commenced the use of the iron, he had one severe paroxysm. He remained in town about ten days, during which time he was perfectly free of pain. Being anxious to return to his parents for a few days, contrary to my wishes, he walked eleven miles to his father's house, and perspired very freely. The next day after his arrival he had a slight attack, and the succeeding day one more severe: he then returned to the city. The medicine was now increased to two scruples every four hours, and for three weeks that he remained under my care, he was perfectly free of the complaint. His father now took him home, supposing him completely cured; I recommended a continuance of the medicine for some time, and to discontinue the use of it gradually. His father was so fully satisfied of the good effect of the rust of iron, as he called it, that he purchased a pound of it at an apothecary's, with a resolution to administer it freely: I have since heard he has had one or two slight attacks, but a few doses of the carbonate has entirely removed them.

Mrs. H—y, a respectable lady of this city, aged 70 years, had been for a number of years troubled with severe pain in her face, which would often intermit for several months; in the fall of 1821 she had a severe attack of remittent fever, and from that time she has been constantly affected, being scarcely an hour of the day free from pain. Having in former attacks received little aid from medicine administered by different physicians, she had little hopes from that source.

In December following I was called to see her, for the purpose of giving her some temporary relief from her sufferings. After administering an emetic of ipecacuanha and a carthartic, she commenced the use of carbonate of iron; two scruples were given three times a day: after continuing the medicine about one week, the pain gradually diminished in violence; at the expiration of twelve days, owing to a soreness of her mouth, the medicine was discontinued. The pain was however removed. She now took an infusion of colombo and bark, and at this time has remained free from the complaint.

ART. IX. SINGULAR CASE OF EXTRA UTERINE CONCEPTION. By Dr. PLEASANT COTTON, *of Waynesborough*, *Georgia.*

THE subject of the above case was a negro woman, aged 30 years. On the night of the 23d of January, 1819, I was called to see her in labour. I found her with slight pains, returning at considerable intervals. I proceeded to an examination, per vaginam, but could ascertain nothing satisfactory in consequence of the distention of the labia pudendi from watery effusion. From the best evidence I could collect, I had no doubt but this was her full term of pregnancy; an enema was thrown up and an anodyne given. The pains did not return. In a day or so, I commenced treating her with large and repeated doses of cream of tartar and jalap, for the relief of dropsy. This treatment was continued with practical advantage until the night of the 4th of February, when I was again called to her in labour. The pains were on the decline, and very shortly after, entirely ceased, and did not again return until death, which ensued five weeks after. This patient had been dropsical during the greater part of her pregnancy, and at my last call, I found her universally anasarcous.

The stomach and bowels had measurably lost their sensibility to the stimulus of the aforenamed cathartic, and for it was substituted calomel and gamboge. This, or some other cathartic, had to be given every other day at least, in order to produce evacuations from the bowels; and without daily discharges, she would be in the most agonizing pain. Her dejections were uniformly of the most morbid appearance, and offensive to the last extreme. For some days previous to death, there was a total suspension of action in the bowels, not being able to procure alvine evacuations by any means whatever.

On examination, *post mortem*, assisted by my friend and colleague, Dr. S. Harlow, we discovered the following facts. In the first place, we drew off from the abdomen three and a half gallons of a very turbid and remarkably offensive fluid. On opening the abdomen, the first thing that presented itself, was the child extending itself across the abdomen, its head in the right, its feet in the left, hypochondriac regions, its back immediately to the umbilicus of the mother. It was as large a child as either of us had ever seen at birth, and perfectly formed. *The funis was of the usual size, about six inches in length, and inserted into the fundus uteri without the intervention of a placenta.* The uterus was about the size of an orange, its coats very much thickened, and indurated, with a small quantity of a thin glassy fluid within its cavity. The abdominal viscera were all diseased, save the bladder. The liver retained its original shape and position, but looked more like a mass of glue than organized animal matter. The spleen had gone into a state of complete decomposition. As to the omentum, there was not the slightest vestige left. The bladder appeared to be the only viscus that had escaped uninjured from this digression in nature. The bowels had firmly adhered in one uniform mass from the stomach to the rectum, and to the posterior and lateral parietes of the abdomen.

REVIEW.

ART. I. AN ELEMENTARY TREATISE *on* MINERALOGY *and* GEOLOGY, *designed for the use of pupils, for persons attending Lectures on these subjects, and as a Companion for Travellers in the United States of America. Illustrated by six plates.* By PARKER CLEAVELAND, Professor of Mathematics and Natural Philosophy, and Lecturer on Chemistry and Mineralogy, in Bowdoin College, &c. &c. &c. 2d Edition, in 2 volumes, 8vo. Boston, 1822.

THE first edition of this work was published in the winter of 1816. Its appearance was hailed with pleasure by the scientific public in this country, from the extensive information it communicated concerning American minerals; and even in Europe, one of the high tribunals of criticism* bestowed on it almost unmixed commendation. When we state that the present edition retains all the excellencies of the former, that it comprises great additions on the mineralogy of this country, and that every discovery in Europe, down to the latest date, is also duly registered, we have, in a few words, sketched its character and its value.

It would be superfluous, after the notices that this work has received, to give a formal analysis of its contents. We shall endeavour to illustrate its value in a somewhat different manner. Before, however, proceeding to this, we must be permitted to offer our tribute of approbation to the author, for retaining the arrangement of minerals, which he adopted in the first edition. Daily experience must convince every mineralogist, that a classification founded on chemical analysis, is the only one that is destined to be permanent. The great promulgator of the Wernerian arrangement in Great Britain, has already abandoned it, and in his later works, substituted that of Mohs. This, he has dignified by the name of the *Natural History Method*, but with what propriety the following observations of Professor Cleaveland will sufficiently illustrate.

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* Edinburgh Review.

"In the arrangement of Mohs and Jameson, the principles, depending on the external characters of minerals, are permitted to have their full operation. It hence results, that different ores of the *same metal* are sometimes widely separated from each other; indeed, several species among the ores of the *metals*, are *actually transferred* to the *earthy* class. Thus, the sulphate, molybdate, chromate, phosphate, and carbonate of lead, the carbonate of iron, and the calcareous oxide of tungsten, &c. are arranged with the carbonate and sulphate of barytes, the carbonate and sulphate of strontian in the order Baryte, the sixth order of *earthy* minerals. The green oxide of uranium, the arseniate of cobalt, the phosphate of iron, the graphite, &c. are arranged with mica, pinite, chlorite, talc, &c. in the order Mica, the third order of *earthy* minerals. In fine, *one variety* of arseniate of copper is placed in the third order of *earthy* minerals, while the *other varieties* of arseniate of copper are found in the fourth order of *earthy* minerals, called Malachite, which also contains the phosphate and carbonate of copper." Page 83.

It is hardly probable, that any thing but the distinguished services of Professor Jameson, in the science of mineralogy, can give more than a bare currency to a system, which thus contravenes some of the first principles of classification.

The error in the systems both of Werner and Mohs, seems to be the undue preference they give to the external characters of minerals, in deciding the species. They should be studied and understood, but not solely relied on. The views of Professor Cleaveland on this subject, deserve the praise of eclectic skill, and are worthy of recapitulation.

1. In establishing a scientific arrangement of minerals, their true composition is the only true criterion for determining the species, and *when known, should be employed in all cases*. 2. When the composition of minerals is entirely un-

known, or but imperfectly understood, other characters depending more or less on the composition, must be employed, and of these the more important are derived from the *crystalline form and structure.* The primitive form of crystals, should be attentively examined. 3. When all assistance from analysis, or the crystalline form, is denied, the species must be determined by a well chosen aggregate of those *external characters*, such as structure, fracture, hardness, &c. which depend most intimately on the nature of the mineral. *It is, however, to be understood, that in all cases, where the composition is unknown, the species are to be considered provisional, till the progress of chemistry shall enable us to re-examine them.* Pages 88, 89.*

The ardour and success with which the science of Mineralogy has been cultivated, during the period embraced between the publication of the different editions of Professor Cleaveland, is truly remarkable. Nothing will give a better idea on this point, than merely subjoining a list of the minerals added in this second edition. It is to be remembered, that with very few exceptions, they have all been discovered within the space of five years.

To the first class, there is added,

Barystrontianite,
Blue Vesuvian Limestone,
Sub Sulphate of Alumine,
Sub Sulphate of Alumine and Potash,
Polyhalite,
Crystallized Carbonate of Magnesia,
Pulverulent do. do.

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* The interesting and popular work on Mineralogy, by Mr. William Phillips, which has been re-printed in this country, with NOTES, is arranged on a similar plan with that of the system under consideration, and may be perused with advantage. In Aikin's Manual, [also re-published,] the chemical arrangement is generally pursued, while in the synoptical tables, the minerals are grouped according to certain chemical or external characters.

To the second Class,

Michaelite,
Leelite,
Bucholzite,
Gehlenite,
Siliceous Feldspar,
Ice Spar,
Necronite
Peliom,
Steinheilite,
Indianite,
Romanzovit,
Freisleben,
Haydenite,
Helvin,
Humite,
Grenatite,
The varieties of Zeolite,
Karpholite,
Pargasite,
Pyrallolite,
Native Lime,
Allophane,
Lenzinite
Amblygonite,
Cereolite
Chlorophæite,
Desmine,
Fahlunite,
Sphærulite,
Thulite,
Eudialyte,
Gibbsite,
Maclureite.

To the third Class,

Fossil Copal,
Dysodile,
Hatchetine.

And to the fourth Class,

Cupreous Seleniuret of Silver,
Eukairite,
Tennantite,
Seleniuret of Copper,
Brown Copper,
Stilpnosiderite,
Blue Iron Stone,
Hedenbergite,
Skorodite,
Pyrosmalite,
Cobaltic Sulphuret of Lead,
Plombgomme,
Tungstate of Lead,
Franklinite,
Siliceous Oxide of Manganese,
Oxide of Chrome,
Oxide of Molybdena,
Yellow Oxide of Tungsten,
Crichtonite,
Orthite,
Pyrorthite,
Fluate of Cerium,
Yttrocerite

Two new metals have also been discovered, viz. Selenium and Cadmium.

The most interesting view, however, in which this work can be presented to our readers, is in its illustration of the progress of the science in this country. When the first edition was published, it was remarked by the Edinburgh Reviewers, (vol. 30, p. 387, American edition,) that with the exception of the *Red Oxide of Zinc and the Native Magnesia*, (Hydrate of Magnesia,) both of them discovered by Dr. Bruce, "no simple minerals had hitherto been noticed in the United States, that were not already known to exist in other parts of the world." Five years have scarcely elapsed, and the list is more than doubled. We can now add,

The *Marmolite* of Nuttall.

The *Yellow Oxide of Tungsten*, discovered by Professor Silliman.

The *Franklinite.*

The *Necronite*, discovered by Mr. Hayden.

The *Jeffersonite*, discovered by Professors Vanuxem and Keating.

The *Green Oxide of Zinc*, from Ancram, ascertained by Dr. Torrey.

The *Maclurite* of Seybert and Nuttall.*

The *Gibbsite*,† ascertained by Dr. Torrey and Professor Dewey, (provided the Wavellite be deemed a Phosphate of Alumine ;) and we may also subjoin,

Siliceous Felspar, unless the Albite is considered the same mineral. This was discovered by Dr. Hunt, and declared a new variety by Col. Gibbs.

Nemalite, described by Mr. Nuttall.

The *Crystalized* Carbonate of Magnesia, discovered by Mr. Pierce.

The *Haydenite*? (Cleaveland, p. 478.)

We have also been struck with the great variety, as well as number, of minerals found in the United States, when compared with the rest of the world. A comparative abstract drawn from this work, will prove, in a most striking manner, how much has been done in the science during the short space of the last ten or twelve years.

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* New-York Med. and Phys. Journal, vol. 1, p. 202.

† Ibid, vol. 1, p. 68.

In the first class, out of sixty species and important subspecies, thirty-three have been found in the United States.

In the second class, out of about two hundred species and sub-species, ninety-eight are found in this country.

In the third class, eleven out of eighteen.

And in the fourth class, sixty-six out of one hundred and sixteen. Making a total of two hundred and eight species and sub-species, out of three hundred and ninety-four: and in this comparison of the *United States, against the world*, it must be observed, that the minerals peculiar to oriental and volcanic countries will probably never be found in them. It is certainly, therefore, not a vain boast to observe, that a most laudable as well as successful spirit of research has distinguished the mineralogists of the present day amongst us.

All the metals have been found in the United States, except Platina, and those alloying it, Tin, Cerium, Selenium and Cadmium. We possess all the ores of Zinc, three indeed of which are as yet peculiar to us; all the known ores of Tungsten, and all the valuable ores of Iron.

The accuracy and neatness with which this work is executed, are deserving of great commendation. We have noticed but few errors, in addition to those mentioned by the author. In p. 145, however, the name *De Runey*, should be, we believe, *De Russey*. Some remarks have also occurred to us, in turning over the pages, which will best be presented, by referring to the species to which they apply.

Page 168. *Compact Limestone.* In the first edition, Professor Cleaveland states that a shell marble is found at Marbletown, (Ulster county, N. Y.) This is given on the authority of Rees' Cyclopedia, (1st. edit. p. 162.) In the second edition, this notice is omitted. The locality is, however, correct, though it would be more minutely accurate to refer it to Kingston, in the same county.

About two years since, marble of a similar nature was found in Otsego county, in this state, and has been used to a considerable extent.

Page 775. *Ibid.* The expression "it contains, according to Mr. White," would be more accurate, if worded thus, "It contains, according to Dr. Hadley." See the notice in Silliman's Journal, vol. 3.

Page 212. *Gypseous Alabaster*, according to the distinction proposed by Professor Cleaveland, of a compact quality, and susceptible of a beautiful polish, was discovered in 1821 at Sandusky, in the state of Ohio.

Page 235. The *Limpid Quartz*, from the West Canada Creek, is found on the *banks* of this stream. We have understood from good authority, that no crystals have been observed in the sands, but they have been collected in great profusion from the grounds contiguous to this branch of the Mohawk. They are generally found in cavities in quartzy stones, similar to those which of late have been brought from lake George.

Page 212. *Gypsum.* We have read with pleasure the following information. "Experiments in New-York and other states, have shown that gypsum may be employed with success in the immediate vicinity of salt water." Should this prove to be correct, the agriculture of some of the southern counties in this state will have received a most important aid. We had however understood, that all experiments with it, particularly on Long Island, had totally failed.

Page 131. *Common Salt.* The springs in the state of New-York, are stated, on the authority of Col. Gibbs, to furnish more than 500,000 bushels annually. It may be interesting to mention the exact number of bushels manufactured during the last three or four years, as taken from the official reports. Quarterly returns are made to the public offices, and from these it appears, that there were manufactured,

From Aug. 6, 1817 to Feb. 4, 1818, 106.091 bushels.
From Feb. 1818 to Feb. 1819, 425.761 do.

From Feb. 1819 to Feb. 1820, 548.592 1-4 do.
From Feb. 1820 to Feb. 1821, 567.337 do.
From Feb. 1821 to Feb. 1822, 476.129 do.

Coal. In the first edition, (page 410) there is this observation: " *There are indications of Coal in New-York.*" In the second edition it is omitted, and, we fear, with too much propriety. Within a few weeks, it has been stated that coal of a good quality has been observed in Livingston county. We hope that it is the precursor of future discoveries.

Graphite. (p. 504.) The locality of this mineral should be, we believe, Ticonderoga, as specimens of a large size and excellent quality, have from time to time been procured from that place. This is with reference to the localities in the northern part of this state. At all events, it should be added to those already inserted.

Page 156. *Verde Antico*, or *Green Antique Marble.* Dr. E. D. Clarke appears to have discovered the locality of this at Ampelakia, on Mount Ossa. (See his Travels, part 2, vol. 7, p. 359, 4th edit.) The articles on Tellurium and Gold, so far as they relate to Transylvania and Hungary, might also be usefully revised by a reference to this author, who is, we believe, the latest scientific traveller that has published his observations on those countries.

Professor Cleaveland, in the conclusion of his preface, intimates, that he had collected numerous facts for the Geological part of his work, but it was found, that they could not be inserted without unduly increasing the size of the book. "They must, therefore," he observes, "if ever published, be left for a separate volume." We hope that, after the favour with which the present work has been received, the author will not delay conferring another obligation on the scientific public, by preparing an elementary Treatise on Geology. Such a work is particularly needed, and it should contain copious illustrations and references to districts of country that are readily accessible to the students of science. We venture also to suggest the great utility of adding to such a work, a full account of Organic Relics. These should be arranged according to the latest systems, combining the discoveries both of French and English geologists. And above all, they should be accompanied with plates to guide the observer in his investigations. However much it may be ridiculed, yet the examination of organic relics is a subject of inquiry to many individuals who are not directly engaged in scientific pursuits; and a treatise, such as we have mentioned, would be gratefully received by them. Again, the works of Parkinson and Cuvier, and the Philosophical Transactions, together with several others, now in the course of publication in Europe, form the works of reference on this subject. The price of these is such, as almost to prevent any private individual, with moderate means, from obtaining them. We hope, therefore, that a work of this description will shortly be presented to the American public, by an individual *competent to the undertaking*. That Professor Cleaveland is such a person, we have an earnest in the ability with which the present work is executed.

T. R. B.

ART. II. BOYLSTON MEDICAL PRIZE DISSERTATIONS, *for the years* 1819 *and* 1821. *Experiments and observations on the communication between the stomach and the urinary organs, and on the propriety of administering medicine by injection into the veins.* By E. HALE, Jun. M. D. M. S. S. Boston, 8vo. pp. 135. 1821.

DR. HALE is already advantageously known, as having contributed several interesting facts to our stock of physiological knowledge, and as the author of a treatise on the Spotted Fever, a work of much greater extent than the one now before us. On the present occasion he appears as a competitor for the Boylston premium, and has proved himself a successful candidate. according to the judgment of the

Boylston committee, on two questions proposed by them, and which have been long agitated by experimental philosophers.

We shall, without further introduction, say something on each of these subjects, and reserve for ourselves the pleasure, at a future day, to lay before our readers some account of the author's work on the late spotted fever.

We shall say nothing at present concerning the opinions of Hippocrates, Galen, and several others, of the communications which they supposed to exist between the liver and the stomach, and the urinary organs.

The copious discharge of urine from the bladder, almost immediately after taking large quantities of drink into the stomach, is a fact familiarly known to every person, but one that has never been satisfactorily accounted for.

The natural explanation which has always suggested itself, is, that there must necessarily be some other and more direct communication between the stomach and bladder, than that of the round of the circulating fluids. There is reason to believe that even Hippocrates suspected this. Galen also was particularly struck with this phenomenon, and thus ingeniously endeavours to explain it.

> "Quippe in libris de naturalibus functionibus jam ostensum est, jecur ex ventriculo per mesentericas venas, velut arbores a terra per radices, cibum ad sese allicere; sic etiam renes id, quod aquosum, in sanguine est."

We are not sure that the author of the essay before us is correct in interpreting this passage to mean blood vessels passing between the kidneys and stomach. It is more probable from the language, that Galen meant another class of vessels, which, by a direct communication, conveyed off the watery part from the blood, in contradistinction to the mesenteric veins, which carried the more solid and nutricious matters of the intestinal tube into the liver, and thence into the circulation. Possibly he may have seen lymphatic vessels passing between these organs. We cannot conceive that he alluded to the emulgent veins and arteries.

The hypotheses of Fabricius, Darwin, and others, since the time of Galen, are not worthy of being remembered. With regard to their experiments, which are equally unsatisfactory, the error was, that they seemed to be persuaded of the existence of an immediate communication from the stomach to the bladder, instead of directing their researches, as Bartholinus more rationally did, to some connection of this kind, between the stomach and kidneys, or as Sir Everard Home has since done, to the anatomical relations which subsist between the stomach and the blood vessels. By grasping at too much, they missed the true secret: the honour of discovering, or at least of opening the way for such discovery, has been left for Sir E. Home. He has rendered it almost certain, by his neat and judicious experiments, that a large portion of the fluids taken into the stomach, are absorbed directly into the blood.

Our author has treated his subject very methodically, commencing with the history, and finishing with the experiments and results. Dr. Hale remarks,

> "Until Sir Everard Home's experiments in 1808, it was doubtful whether the speedy appearance of the urine in these cases was not the effect of a sympathetic excitement of the urinary organs, by the presence of a large quantity of liquid in the contiguous viscera, rather than of the passage of the same portions of fluid into those organs. Indeed, it is not even now decided that this is not sometimes the case. But Mr. Home has shown that the same portion of fluid which is received into the stomach, will, under some circumstances, pass into the bladder, in a very short space of time." p. 26.

Absurd as this idea may appear to be, it is still maintained by many reputable physicians in this enlightened age. It is even contended that the *odours* of certain substances, taken into the stomach, and detected in the secretions of the mammæ, skin and kidneys, may be explained on the same principle. (See Chapman's Journal; review of Dyckman's Pathology of the Fluids.)

We cannot justify the author in declining to examine this subject anatomically; for after all that can be said in favour of physiology, minute and close dissection, is the basis upon which our reasonings and experiments on this subject must ultimately rest. We could wish that this branch of medical science were more seriously attended to in our different universites, and that more ample provisions were made for zootomical inspections. The great school of Paris would serve as a fine model for us, and might indeed be copied by the colleges of many European capitals, which nevertheless are not sensible, apparently, how much behind-hand they are in this respect to the French metropolis.

> "In pursuing the investigation of this subject, it is not my intention to seek the passage in question, by any *anatomical* discoveries. After the laborious researches of Diembroeck, Ruysch, Malpighi, Lewenhoeck, and other eminent anatomists, it would be presumptuous for me to expect to find what they could not find; or to regard my inability to discover it, as any additional proof of its non-existence. It is in this view, that I am disposed to say with Ruysch, in the words of my motto, 'Oculi mei non adeo Lyncei, ut hunc nodum solvere queam.' My researches, therefore, have been purely physiological." p. 38.

We now come to the experiments of the author. He seems to have been well aware of the nature of the task before him.

> "The first object," says he, "must be to ascertain, so far as it is practicable to do it, what length of time it would require, for fluids to be carried from the stomach to the bladder, through the medium of the circulation. This can never be determined with any considerable degree of precision; yet some approach to the knowledge desired may be made by estimating the rapidity and force of the blood in circulation.
>
> "The whole quantity of blood actually in circulation in an adult,

cannot be estimated at more than from thirty to forty pounds, and is probably less. The quantity which the heart is capable of throwing out at each contraction of the ventricles, is said, by Haller, to be about two ounces; and there are in a middle aged person, at least, seventy pulsations in a minute. Making this the basis of our calculation, supposing there are forty pounds of blood, it will take four minutes and thirty-four seconds, to complete one entire revolution of any given portion of the blood, including its course through the lungs. If the quantity of blood be only thirty pounds, the same revolution will be accomplished in three minutes and twenty-five seconds. But it is not certain that the heart actually throws out at each contraction, the whole quantity of which it is capable. Suppose it to throw out only three fourths of the full quantity, making an ounce and a half at a pulsation; in this case, six minutes and fifty-two seconds will be required to complete the circulation of forty pounds of blood; or five minutes and one second for thirty pounds.

"It is to be remembered, however, that the course from the place where the fluids of the stomach enter the blood, to the kidneys, is very far short of a complete circulation through the whole body. I shall here take it for granted, that the absorption of fluids directly from the stomach, is sufficiently established by Mr. Home. The distance from the stomach to the kidneys, through the circulation, then, is that from the stomach to the heart, through the lungs, back to the left side of the heart, and from the mouth of the aorta to the terminations of the emulgent arteries. This, at the most, cannot be supposed to exceed one half of an entire revolution in the circulation; and is probably less than one fourth. The time requisite, therefore, as far as the circulation is concerned, for fluids to pass from the stomach to the kidneys, is not more than from two to three and a half minutes; leaving the remainder of the time, which actually intervenes between their being taken in drink, and ejected in urine, to their absorption in the stomach, and their secretion and excretion in the kidneys and bladder.

"I do not pretend that this calculation gives with precision the length of time required for the purposes mentioned in it. But I am persuaded that the errors are upon the side of allowing too much, rather than too little time. Neither do I rely upon it as evidence that the fluids actually do pass through the blood, to go from the stomach to the bladder. I bring it forward here merely to show, that from the great rapidity of the circulation, the supposition of their passing in this way is not so absurd, as it seems generally to have been considered.

" We have no data, by which to determine with accuracy how long the kidneys are occupied, under different circumstances, in accomplishing their secretion. Yet these are some facts, which go to show that they sometimes perform it very quickly ; and there is no reason to suppose that the urine is detained in the pelves or ureters, or necessarily in the bladder, after it is secreted. If a person is suddenly exposed to violent cold, or to fright, or anxiety, very soon after having voided his urine, it often happens, that he will almost immediately pass a very considerable quantity more, which will be watery and colourless, without his having taken any fluid in the mean time. As this last urine is entirely different in its appearance, from that which preceded it, it must have been secreted after that was ejected. Indeed the structure of the kidneys, and the size of their vessels, seem adapted to the secretion of a large quantity of fluid in a short space of time ; especially when they are stimulated to more than usual activity, by the substances brought to them in the blood, such as are the fluids which admit of the most ready detection in the urine. The emulgent arteries, as Boerhaave observes, are much larger in proportion to the size of the organs they supply, than those of any of the other viscera.

" We have next to inquire, what length of time is consumed in the absorption of the fluid from the stomach into the blood. But here we have nothing to guide us in the inquiry. I will only remark, therefore, that the theory of a direct passage to the bladder is equally subject to the same objection. For, as it is not pretended that there is any considerable vessel, by which fluids are conveyed from one viscus to the other, they must necessarily be absorbed ; and no reason can be given why they should not be absorbed as speedily through the vessels which convey them directly into the blood, as in any other way.

" So far, then, as any reliance can be placed upon this calculation, it appears that the course from the stomach to the bladder, through the circulation, is hardly less direct and open, than any which can possibly be supposed to exist. That it is sufficiently direct to admit of an explanation of the phenomena, is all that is necessary for me to show.

" I will now proceed to the experiments which I have performed, for the purpose of ascertaining whether, in fact, there is any other course by which the fluids of the stomach may pass off by urine." p. 39—43.

We regret that the flattering expectations which we had been induced to form from these well written prolegomena, have not been realized. None of the experiments were conducted with that precision which was due to their importance, and of which the subject was so eminently capable. We are very much inclined to believe that the admixture of cider, cinnamon, spirits, and opium, with the tinctures upon which the chemical tests were afterwards to be applied, may have materially affected the results of the experiments. This much we know, from our own personal experience, that coffee, which, it appears, the author regularly took with his roll every morning for breakfast, and only three or four hours before he began his experiments, sometimes manifests itself in the urine by a *strong odour* and yellow colour within twenty or thirty minutes from the time it is drank, especially if taken in more than ordinary quantities. Whether any of the changes effected in the colour of the author's urine, by the test of potash, are to be attributed to the affinity between the alkali and the coffee, or between the alkali and the tincture of rhubarb he drank, or to both, is a question which must remain in doubt. It would assuredly have been better to have omitted taking the coffee at breakfast, to say nothing of the oil of cinnamon, rum and cider, which he drank, with the rhubarb, to take off its unpalatable flavour. We are a little surprised, also, that he did not provide himself with an assistant, to bleed him at the proper intervals, as this part of the process had an important bearing upon the subject under discussion. To show that we have not misrepresented Dr. Hale, we will refer the reader, as a sample, to Experiment III.

Upon the whole, Dr. Hale deserves praise for what he has done, though his experiments, in fact, are a close imitation of those of Sir E. Home. The following are the conclusions to which the author arrives, and which may be taken rather as a statement of the grounds of the argument up to the present time, than as the result of his own discoveries.

" The following conclusions appear to result from the facts observed in the foregoing experiments.

" 1. The speedy discharge of watery urine after taking a large quantity of liquid in drink, is not occasioned by sympathetic excitement of the urinary organs, but by the actual excretion of the fluid received.

" 2. The same portions of fluid, which are received into the stomach, begin, under certain circumstances at least, to be collected in the bladder within twenty minutes from the time when they are taken.

" 3. When a large quantity of fluid is taken, the excretion of urine is greatly increased; but the increase does not bear any very exact proportion to the quantity of drink. The increased discharge begins in about twenty minutes, is at its height in about an hour, and terminates generally in less than two hours.

" 4. When a liquid which is coloured with rhubarb is taken, the colour of the rhubarb appears in the urine; and its appearance in that excretion is not at all confined to the time of the increased discharge, which is occasioned by the quantity of fluids received.

" 5. When a liquid, which is of such a nature as to admit of ready detection in the animal fluids, is taken into the stomach, if the quantity is sufficiently large to affect the whole mass of blood, it will be found in the blood drawn from the veins as soon as in the urine; although it will continue to appear in the urine after it has disappeared from the blood.

" 6. When a quantity of colouring matter sufficient to affect the whole mass of blood is taken, mixed with a large quantity of liquid, the colour appears in the urine as soon, at least, as the excretion *begins* to be increased, and is at the deepest before the increase of urine is at its height; but if the quantity of colouring matter be only a little more than sufficient to tinge the amount of liquids taken, it does not appear in the urine until after a quantity of watery urine has been passed, and does not give its deepest colour until after the excretion has diminished." pp. 56, &c.

The point which the author appears to have been most desirous of ascertaining, and which undoubtedly was of great moment, was, whether the fluids taken into the stomach, would be proved to have entered simultaneously, or nearly so, into the blood and the bladder.

" It is much to be regretted that neither of these gentlemen, [Home and Darwin,] has told us, with more precision, what length of time intervened between drinking the liquor which was sought in the blood, and drawing the blood for examination. It is at least probable that this circumstance alone would explain most of the difference of their results. Two drachms of nitre are not sufficient to impregnate the whole mass of blood, supposing it to have been mixed with that fluid; and therefore the whole quantity might have passed through the veins, and yet have escaped detection, until it was separated from the blood by the kidneys, and appeared in the urine. It will be seen by the experiments which follow, that this explanation does not rest merely upon hypothesis, or conjecture." pp. 36, 37.

That he has failed in this attempt, and that his experiments, therefore, did not arrive at results more definite than those of Sir E. Home, is evident from what follows. It was not until Experiment III. that he first undertook to determine the changes effected in the composition of the blood.

" In twenty minutes, three fourths of an ounce was discharged. It was of a light yellowish colour, and was changed by the test to a dull reddish yellow. As soon as possible, I opened the median vein of the left arm, and took two and a quarter ounces of blood. This was completed in less than five minutes." p. 47.

Again—

" In thirty-five minutes, I attempted to take more blood from the orifice in the vein, but without success.

" In forty minutes, voided two ounces of urine. Its colour was a bright yellow, rather lighter than the last, very clear, and was changed by the test to a clear wine-red. Immediately after, I opened the basilic vein, and took an ounce of blood. This was accomplished in less than two minutes." p. 48.

In Experiment V. and last, before he drank the rhubarb—

" I opened the median vein of my left arm, and took an ounce of blood, to serve as a standard of comparison. At twelve o'clock I drank undiluted an ounce of tincture of rhubarb, in which were thirty drops of tincture of opium; and to cover the taste, I took, at the same time, three or four drops of oil of cinnamon, in a very little common spirit." p. 55.

Again, after the draught, he says,

" In twenty minutes, voided a drachm and a half. It was obviously coloured by the rhubarb, and was changed very considerably by the alkali. Immediately after I made another opening in the median vein, and took three ounces of blood. This was accomplished in less than two minutes." p. 55.

Again—

" At six o'clock, [i. e. 6 hours after he took the rhubarb,] voided an ounce and three quarters. It was evidently tinged by the rhubarb, but less so than that which had been previously discharged. I immediately opened the median basilic vein, and took two ounces of blood." p. 55.

It was therefore only in two of the five experiments that Dr. Hale pretended to examine the blood; but what is very remarkable, neglected to apply his tests to it until the succeeding days, when it had necessarily undergone more or less decomposition, and was consequently unfit for experiment. In Experiment III. he says,

"The next day I carefully examined the blood which had been drawn. The serum of the two first portions was evidently tinged by the rhubarb; that of the second, more so than the first. It left the peculiar stain of rhubarb upon the glass. The colour of both was a little deepened by the test. The quantity of the third portion was too small to permit me to decide with confidence; but it appeared to be more deeply coloured than either of the others." p. 49, 50.

And in Experiment V.

"The next day, I compared the serum of the blood, which was drawn at the different times. That which was taken at twenty minutes, was very considerably coloured by the rhubarb, and received a deeper tinge by adding the alkali. The colour of this portion very nearly resembled that produced by two drops of the tincture of rhubarb in a drachm of pure serum, and was nearly as much changed by the test. The serum of the blood taken at six o'clock was not at all tinged, but appeared, in every respect, like that drawn before the rhubarb was taken." p. 56.

Whatever changes, therefore, may have been produced in the blood by the tests at this late period, cannot be relied upon with implicit confidence. Dr. Hale should have left no room for doubt or cavil, when so excellent an opportunity was afforded him of determining with exactitude, that which he was in search of. We are aware that he had taken the precaution in the fifth experiment, to compare the blood drawn from the arm before the draught, with that which he drew from the arm after it, and that the application of the tests to these different portions, was followed by different results. But allowing that no decomposition had taken place in the space of twenty-four hours, or that there could have been no fallacy in the results produced by the tests, assuredly it must be confessed that one single experiment is not sufficient to demonstrate the truth of the theory.

The following paragraph explains the object of the second dissertation.

"It sometimes happens in the course of medical practice, that a particular medicine is strongly indicated, at the same time that there is some powerful obstacle in the way of administering it. These cases are indeed rare; but when they occur, they are of a most serious nature, and call very urgently for some mode of relief. A mechanical obstruction in the œsophagus might perhaps be removed by vomiting, were it not that this same obstruction renders it impossible for an emetic to be taken: and still more frequently, the operation of a carthartic is prevented, when it is very much needed, in consequence of an irritable state of the stomach, which rejects every medicine that it receives. Hence we are led to the inquiry, whether some other mode of introducing medicines into the system be not practicable, and particularly whether this may not be done by injecting them into the veins." p. 71.

After some unsuccessful experiments upon dogs by Dr., afterwards Sir Christopher Wren, and by Signior Fracasti, attempts, we are informed, were made at Dantzic about the year 1667, by Mess. Fabricius, Smith, and Scheffler, to ascertain the effect of medicines when injected into the veins of the human subject. The medicines were, laxatives and alteratives; and the diseases, which are said to have been syphilis, epilepsies, gout, apoplexy, and plica polonica, are reported also to have been miraculously cured, with the exception of two out of the eight, who died under the operation of the experiments.

Besides the deleterious effect of air introduced into the blood vessels,

"Another circumstance which seems to show that the excitability of the different parts of the circulating system is peculiar to themselves, and that there is danger in disturbing the regularity of their actions, is the effect produced by the introduction of re-nous blood into the arterial system. If the consequences are so fatal, which follows the application of a fluid so similar to the natural one, as to elude all attempts to detect any difference, except in colour, how much more injurious must be supposed to be the action of foreign substances, which have been scarcely at all assimilated to the animal system.

" Another danger to be apprehended from the injections into the veins, arising from their peculiar irritability, is an inflammation of the internal coat of the vein. We are taught in surgical treatises, that very severe, and even fatal inflammation, has sometimes been excited in a vein, by puncturing it with a dull or rusty lancet, in bleeding. Such occurences are doubtless very uncommon. But if they can ever be produced by so slight a cause, there would be much more reason to fear them, from the irritation which would be unavoidable in introducing foreign substances into the vein. This danger would be greatly increased, if the substance introduced were of an irritating nature, as most medical substances must be ; especially when applied to so delicate a structure as that of the internal coats of the veins." p. 78—79.

Nothing was done in this department in physiology, until the experiments of Fontana, Brodie, Magendie and Orfila, which besides being contradictory, only go to show, (and which in truth proves very little on this subject,) that these injections have occasionally been practised with safety upon pigeons, rabbits, guinea pigs, and some other species of the smaller animals. (p. 80, &c.) Contrary to the opinion which has been commonly entertained, that medicinal substances differ in their effects according as they are admitted into the blood, or into the first passages, the general tendency of these experiments is,

" To lead to the belief that the effects of medicinal substances are not materially different, in consequence of the modes of their introduction into the system." p. 84.

Again, says our author,

" It is sufficiently evident, however, in regard to many substances, that the mode in which they are introduced into the system does not materially change the nature of their action upon it, except in respect to its violence." p. 95.

If this, then, be true, and if it be also certain that the same substances which are poisons to some animals, are innocuous to others, (p. 93,) and that the experiments which have been made by different physiologists, are, for the most part, contradictory, or enveloped in doubt, (p. 94,) by what motive was our author induced to proceed in this unprofitable investigation, or by what process of reasoning did he arrive at the following conclusion?

"From a very general view of the subject, founded upon the observations of Orfila and others, I was strongly inclined to the opinion, that the medicinal substances in question may, in some cases, be advantageously introduced into the system, by other means than by the stomach." p. 96.

Useless, it would seem, as these researches are represented to be by the author, he nevertheless persisted in undertaking them, which does not perhaps make it to be regretted that he was foiled in almost every experiment; though he appears to have bestowed much more labour on this subject, and to have pursued it much farther, than that which had previously engaged his inquiries. Whether the substances he used were too inert, or the instruments he employed too obtuse; or whether his apparatus for confining the rabbits was too complicated, or that he did not follow closely enough the track of his predecessors, it is impossible to say; all that we know is, that there seemed to be constantly in the way some failure or mistake to defeat his purposes. At one time the veins were too small, or too much lacerated; at another the scalpel slipped from his hand and he cut the artery; at another, the liquid in the syringe was nearly all spilt. Occasionally there did not appear to be on hand a plentiful supply of rabbits; and now and then, the author was unfortunately taken ill, and could not attend to the progress of the experiments. The following quotations explain our meaning.

"In consequence of some minor difficulties in this part of the operation, a part of the oil was lost, and not more than two thirds of it was introduced into the vein."

"But at about two in the afternoon, it was found dead, and a large quantity of fresh blood upon the floor beside it. I was ill at the time, and was not able to examine this animal until several days after its death; in the mean time it had received some injury, which rendered it impracticable to ascertain with precision the state of the wound, and of the parts concerned."

"I attempted to make an injection into the crural vein of a rabbit, and made an incision for that purpose a little above the knee; but I found the vein too small to admit the tube. While I was trying to introduce it, the animal made a sudden start, and I wounded the artery."

"The following experiment was made upon the same pair of rabbits as the first, but the operations were reversed, the medicine being injected into the vein of the male, and the female receiving it into the stomach."

"The tube was introduced, and the infusion of rhubarb slowly injected into the vein. This was done with some difficulty, in consequence of the smallness of the tube; and a part of the infusion was lost."

"I laid bare the crural vein of a rabbit near the knee joint, and passed a ligature under it. I then made an opening into it, and attempted to introduce the tube, for the purpose of injecting an infusion of ipecacuanha; but the vein was too small to admit the tube. After several trials, I was obliged to abandon the attempt. In these trials, partly in consequence of the struggles of the animal, the vein was considerably injured, and was split so, that the tube passed through it."

"I prepared an infusion of colocynth, by pouring two ounces of boiling water upon one drachm of powdered colocynth, which was filtered after twenty-four hours. Two drachms by measure of this infusion was given to a rabbit, at twelve o'clock. As no effect was produced by it, at a quarter past three in the afternoon, I gave four drachms more of the same infusion. Still the medicine produced no effect."

"I gave to a rabbit four drachms by measure, of the infusion of rhubarb before mentioned, [Experiment III,] but it had no apparent effect whatever."

"To another rabbit I gave one scruple of ipecacuanha in powder, mixed with three drachms of water. Half an hour after, I gave as much more in the same quantity of water. I could not perceive that it produced the least effect upon the animal."

"The next morning he was found dead. He was entirely stiff, and appeared to have been dead several hours. I was prevented, by the preparations for the following experiment, from dissecting it at the time; and was afterwards too ill to do it, until it was too late to derive any benefit from the dissection." pp. 101—111.

There was one experiment, however, [the tenth] made by our author, which, though it will never probably be followed by any useful results, is calculated to redeem all these blunders, inasmuch as it reflects the highest honour upon his moral bravery and his bold zeal in the cause of truth; being an experiment that has been projected time out of mind, but always deemed too hazardous to perform. Dr. Hale, an American, we are proud to say, is the first who has had the courage to perform on his own person, the dangerous experiment of injecting foreign substances into the veins of his body. There is a spirit of chivalry and daring in this act, which would have done honour to the most ardent enthusiast of experimental inquiry, and will, doubtless, cause the author to be recorded, by the medical historian of future times, among those few illustrious men, who, like Russel, Desgenettes, Whytte, Larrey, Bichat, and Valli, with undaunted heroism have braved the terrors of pestilence or nobly sacrificed their lives in determining its laws. We believe that this experiment stands without a parallel, and that it is one which, by the curious information it has disclosed, deserves to be ranked in the light of a discovery. It will always make the book of Dr. Hale much sought after, and a point of reference for future physiologists, serving like the amber which encrusted the remains of an insect, to carry down to posterity the adventitious matters with which it is associated.

We must be pardoned by the reader, for inserting the account of this experiment at length.

"Having been persuaded, from my own observations, and those of others, that some of the milder medicines may be injected into the veins with safety, I resolved to make the experiment upon myself. Accordingly I filled a half ounce measure with cold-pressed castor oil, and placed it in a basin of water of the temperature of 100°; one drachm of it, I drew into a syringe, and placed this also in the basin; it being my intention first to inject one drachm, and if I should feel no inconvenience from it, to continue the injection of the remainder of the half ounce. When I had made all the preparations which I thought necessary, I sat down and counted my pulse, and found it to beat eighty times a minute. I was in good health, but could not avoid some little agitation and excitement of feeling at the novelty and uncertainty of an experiment upon myself, which, so far as I knew, had never been attempted upon any human being; and this had a little quickened my pulse. An assistant [a medical student] placed a ligature round my left arm, as in the common operation for bleeding, and opened the median vein by a pretty large orifice; taking particular care that the opening in the vein and the skin should be exactly opposite. He then attempted to introduce the silver tube, while I held a bowl to receive the blood, which flowed very freely. But being a little agitated, he was not able to get the tube into the orifice in the vein. As there was no time to be lost, I took the tube myself, and after several ineffectual trials, which gave considerable pain, I succeeded in introducing it. We immediately took off the ligature, and began to inject the oil. The hæmorrhage ceased as soon as the ligature was loosened: we estimated that about eight ounces of blood was lost, before that was done.

"The injection of the oil was a slow operation. In consequence of the delay arising from the difficulty of introducing the tube, the temperature of the oil was reduced to about 70°. It was consequently less limpid, and less easily forced through the small tube into the vein. But the principal obstruction arose from the difficulty of carrying the oil forward into the circulation, after it had entered the vein. It was constantly disposed to regurgitate, and to pass out by the side of the tube into the cellular membrane, and upon the arm. To prevent this, we injected the oil very slowly, and as often as a little was collected in the vein, it was necessary to press it forward gently by the fingers upon the arm until it disappeared. While we were injecting the first drachm of oil, I carefully watched my own feelings, and kept my hand upon the region of the heart, that I might be fully sensible of it, if there should be any unusual pulsation ; and after this drachm was all thrown up, we waited two or three minutes to watch its effects before we injected any more. But not perceiving any unnatural sensation, we proceeded to inject the remainder of the half ounce. In doing this about a drachm was lost, by the regurgitation of the oil out of the vein ; and we poured out another drachm and injected it, so as to make up the full quantity that I had originally intended to use.

" It was half past eleven in the morning, when the vein was opened, and we were occupied twenty-five minutes in completing the injection. In order to be certain that the oil actually entered, and to retain as much of it as possible, I carried the tube some distance into the vein ; the greatest part of the time, not less than three quarters of an inch of its length was within the vein. There was no hæmorrhage when the tube was withdrawn. There was a tumour, of nearly the size of half a walnut, below the vein on the inside of the arm, occasioned partly, if not wholly, by an effusion of the oil into the cellular membrane, and increased perhaps by an extravasation of blood with it. A compress of lint was placed upon the wound, and the arm was bandaged as in the common operation of venæsection.

" I felt very well for a short time after the operation was finished. The first unusual sensation that I perceived, was a peculiar feeling, or taste, of oiliness in the mouth, a little after twelve o'clock. Very soon after, while I was washing the blood from my arm and hands, and was talking in very good spirits, I felt a slight nausea with eructations, and some commotion in the bowels, then a singular indescribable feeling seemed very suddenly to ascend to my head. At the same instant, I felt a slight stiffness of the muscles of the face and jaw, which cut short my speaking in the middle of a word, accompanied by a bewildered feeling in my head, and a slight faintness. I sat down, and in a few moments recovered myself a little.

"This part of the experiment had been performed in a room, at a little distance from my lodgings. At a quarter past twelve, I walked home My countenance was pale, and the oily taste continued in my mouth, with some dryness; but I felt a little better for the air. After sitting ten minutes, at twenty-five minutes past twelve, I counted my pulse, and found it seventy-five in a minute.

"*Thirty-five minutes after twelve.* The disturbance in the bowels continues and increases; slight pains moving about in them, with the feeling as if I had taken a cathartic: copious eructations of wind, and slight nausea. There is a strange sensation in my head; it is not a dizziness, though somewhat like it. My arm feels rather stiff, which I attribute to the bandage; it is also a little sore, but is not painful.

"*Forty-five minutes after twelve.* The bowels are still more in commotion, and the nausea is increased. My mouth still feels oily, but is not so dry as before. In five minutes more the disturbance in the bowels was increased so much, that I thought there would have been a discharge from them, and made the attempt; but without success. There is a slight pain in my head, and the strange feeling continues.

"*Twenty minutes past one.* The pain in the bowels is increased, with some tenderness on pressure; have a very strong feeling as if a cathartic were about to operate; but an attempt to procure a discharge from the bowels was unsuccessful, as before. The nausea continues.

"*Two o'clock.* My general feelings are better: the nausea is nearly gone. Notwithstanding my previous experience of the deceptive nature of the pain and uneasiness in the bowels, the feeling of inclination to stool was so strong and distinct, that I was induced to make another attempt, but with no more success than before. The same thing occurred again at forty minutes past two, and twice in the course of the evening. The sensation was, in every instance, strong and perfectly distinct; at least it entirely deceived me, although I was on my guard, from the previous unsuccessful attempts. In the course of the afternoon and evening, the flatulence and pain in the bowels diminished: but the tenderness remained for several days.

" At twenty minutes past two, while I was making a little exertion, my arm began to bleed again, rather freely; and we had some little difficulty in stopping it. I was alone at the time, and before I could call my assistant, and take off my coat and apply the dressings, about six ounces of blood were lost. We were obliged to make the compress and bandage quite tight over the vein; and even then, there was a considerable ecchymosis into the cellular membrane.

" At three o'clock I dined upon a piece of pudding, which I ate with some little relish, but felt a slight nausea afterwards About four o'clock, having a pretty urgent call to visit a patient, I went a little distance in a carriage, but took my assistant with me, to guard against accidents. I did not go out again until the 25th of February, and only twice for a short time till the second of March. Towards evening my arm became quite stiff, and considerably painful, and continued so through the night. It was swollen on the inside, from several inches below the elbow almost to the axilla; and there was considerable heat and tension, especially about the elbow, and a little above it. At eleven o'clock, my pulse was eighty-four. I passed rather a restless night, but got some sleep.

" *February* 21. In the morning there was a small discharge from the bowels, which was smaller and more costive than I am accustomed to have at that hour. My arm was more swelled, and more painful than before; pulse eighty. I had some pain in the head, and was all day much inclined to chilliness, though without true rigors. My arm was quite painful through the day. I had some fever and loss of appetite, and felt altogether too ill to make any use of my faculties, either of body or mind. This state continued several days; and when I began to recover from it, I found my strength so much diminished, that it required two or three weeks to restore it to its former vigour. In order that I might interfere as little as possible with the effects of the injection, I wished to defer making any application to my arm as long as it could prudently be done; but on the evening of the 21st it became so painful, that I applied pieces of linen dipped in the water of acetated litharge, diluted. This answered the purpose, and in some measure relieved the inflammation and pain; and I passed a rather more quiet night.

"*February* 22. I was costive the whole day; and at night took a mild cathartic pill. The pain in my arm was less severe, and I was in every respect better. From this time I gradually recovered my health, although very slowly. I made use of no more medicine, except to continue the lead-water. The swelling of my arm went off slowly, by the diminution of the inflammation, and the absorption of the effused blood; so that on the 25th I was able for the first time to touch my forehead with the end of my fingers. For some time after I recovered my appetite for food, my powers of digestion seemed to be enfeebled, and I was frequently annoyed by indigestion.

"It is now four weeks since the experiment was begun by injecting the oil, and I have not yet quite recovered my usual strength and vigour. I am more easily fatigued, and am obliged to pay more careful attention to my diet than before. My left arm is weaker than it was previous to the operation, and is still sometimes subject to pains about the elbow. The tumour, which was made by the effusion of the oil and blood into the cellular membrane, is not yet wholly absorbed, although it has been constantly diminishing. It is now to the feeling about the size of one half of an English walnut. It does not however materially impede the motions of the arm.

"I have been thus particular in detailing the circumstances attending this disastrous experiment, in order that my readers may judge for themselves, how far the symptoms, which followed the operation upon the vein, were the necessary consequence of the injection, or whether any of them might have arisen from accidental circumstances, or from the manner in which the operation was performed. I am aware of the difficulty of observing correctly one's own feelings, and of the liability to mistake the impressions of the imagination for the effects of disease, or the operation of medicine. But I am not conscious that I have done so in this case."

The remaining four experiments go to show, that irritating substances, like alcohol, calcined magnesia, and Epsom salts, when injected into the jugularsor other veins of small animals, generally produce convulsions and death.

After comparing the experiments of Orfila, Magendie, and others, and weighing the unpleasant consequence which followed the bold attempt of Dr. Hale upon his own person, we feel constrained to say, that he is by no means warranted in coming to the following opinion.

> " From a review of the observations which have been presented, I think it may be regarded as established, that *in general*, the operation of emetics and cathartics on the stomach and bowels is the same when they are injected into a vein, as it is when they are received into the stomach, with the exception that their action is much more speedy and energetic." p. 125.

Rather let us conclude with the more rational remark of the author, that

> " The operation necessary to making an injection of any medicine into a vein, is of too serious a nature, in comparison with taking the same medicine by mouth, ever to come into general use in ordinary cases, even if there were no other objection to it." p. 127.

In finishing our remarks upon the work of Dr. Hale, we cannot forbear to observe, that the style is eminently perspicuous and correct, and free of that bombast and verbosity with which our writings, especially those of our youth, are so apt to be vitiated. It reflects much honour also on the progress of medicine in New England, that the attention of the younger part of the profession is turned to those practical pursuits and useful researches, which are the only sure road to distinction.* T.

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* We have received the above review from a valued correspondent, and we publish it, without wishing to be considered as approving of all the writer has advanced.—Ed.

Medical and Philosophical Intelligence.

DOMESTIC.

Sick Stomach.—A previous part of the present number of our Journal contains a communication on the subject of this singular disorder, which seems to be very prevalent in certain districts of the West. As the facts stated by Dr. Beck, are merely additional to those embraced in certain papers which have lately appeared in the Philadelphia Journal of the Medical and Physical Sciences, and the Western Quarterly Reporter, we present our readers with a detailed account of the disease, as furnished by Dr. Asa Coleman.

"A disease has been prevalent in some of the western counties of the state of Ohio, since their first settlement, which is singular in its symptoms, and frequently fatal in its effects. The country people name it the Sick Stomach, from the sickness and vomiting attending it. This disease is common to a certain extent on the MIAMI RIVER, and all its branches, north of Dayton, particularly on Mad River, and the open country between this and the Scioti river. Some other parts of the Western Country are also subject to it.

"This malady appears to be, in a great measure, confined to particular tracts of country, often of no great extent, and in which there is no peculiarity as to humidity, dryness or elevation, different from the adjoining country where it is not met with; nor does it appear to extend its geographical limits, since the part of country in question has become generally settled. The time at which it is most common, is that of the Summer and Autumn, though it is occasionally seen in all the other seasons.

"In narrating the symptoms, we shall divide the disease into three stages or forms, to render the description more perspicuous.

First stage.—The patient complains of a general weakness and lassitude; pains or soreness in the calves of the legs, sometimes extending up the thighs ; disagreeable sickening sensations are felt at the stomach occasionally ; the appetite becomes somewhat impaired ; sometimes a disagreeable gnawing sensation in the stomach is complained of, especially if food be not taken at the usual time. This sensation appears to be less after eating. He is now easily exhausted by exercise, especially from walking, which increases the pain in the calves of the legs, and produces so much loss of muscular power, that the limbs are not able to support the body, and the patient is compelled to rest. This condition is for the time ameliorated thereby, but will return on a repetition of the exercise, and if this is violent, the disease assumes a more active form. These symptoms will sometimes continue for weeks or months, and disappear without the more active forms of the disease succeeding, while, under other circumstances, they quickly ensue with much violence During the first stage of the disease, the pulse differs but little from the healthy state. It is sometimes rather weak, and slower than natural, or it is quick and tense. The bowels are generally regular.

Second stage.—These symptoms sometimes continue for a few days only, especially if the patient has taken much exercise. What I term the second stage then begins, by an aggravation of the preceding symptoms. The great loathing, nausea, and disagreeable sensation, are much increased, and soon succeeded by vomiting or distressing retching to vomit, without throwing much off the stomach.—The vomiting often returns every hour or two during a whole week together, and the patient experiences some relief after each effort. During this stage of the disease, the strength is much prostrated ; the patient complains of great distress and a burning sensation at the stomach ; the bowels become obstinately costive ; the thirst is frequently great ; hot belchings and hiccough are often troublesome ; the tongue is slightly furred, and the breath has a peculiar, disagreeable smell, resembling that of a person under a mercurial course.

The countenance is generally somewhat flushed, and the patient inclined to be stupid and comatose. There is often dyspnœa, and the respiration is quite laborious.—The pulse is slower than natural—-weak—easily compressed—becoming more frequent and somewhat tense about the third day from the commencement of the vomiting. The extremities are generally inclined to be cold, and the skin is generally as cool as natural, and dry.

The vomiting will often continue six or eight days, the patient remaining unable to take any solid food and liquids are speedily thrown from the stomach. As the patient is somewhat relieved after each spell of vomiting, he falls into a doze. These symptoms will continue, in favourable cases, from the seventh to the ninth day, with greater or less violence, when they suffer some abatement, and the patient is able to retain some food on the stomach, and has generally a very speedy convalescence. Sometimes the disease, instead of taking the abovementioned course, assumes symptoms of a more fatal character, which I shall describe as the third stage or form of the disease.

Third stage.—About the third or fourth day from the commencement of the vomiting, the patient experiences a slight pain in the head; for a short time becomes highly delirious, with continual tossing and moaning. The pulse is rather tense and frequent; the eyes protruded, suffused, sometimes the pupils are dilated; the patient manifesting many symptoms of true phrenitis. In such cases, the vomiting is often not so great, nor the bowels so obstinately constipated. When these symptoms arise, they are apt to destroy the patient in three or four days, if not sooner. In other cases, about the third or fourth day, the patient will fall into a very stupid comatose situation, with a feeble pulse, and symptoms appearing to denote that a gangrene of the stomach has ensued. Such are, the cessation of the distress of the stomach, with occasional vomiting of black, porraceous matter. After the appearance of these symptoms, the patient is generally destroyed in a day or two. Such are the general symptoms and causes of this disease, when not interrupted by medical treatment, which produces variations to a greater or less extent.

The propensity to sleep is often destroyed for several days after an attack of the active form of this disease, and an uncommon degree of wakefulness is experienced. No chilliness occurs at the onset of this complaint, neither during its course, nor does it appear subject to any thing like periodical exacerbations, and in general there is no remarkable indication of superabundance of bile. In some few cases, however, there is a considerable discharge of green bilious matter by vomiting, though generally, the material thrown out of the stomach, consists of liquids that have been swallowed, which are often hot, acrid, or sour, when ejected. From the throat and fauces there is commonly a discharge of thick viscid phlegm.

"From imprudent exposure, those who have had this disease in its active form, are liable to relapses, but perhaps not more so, than in our common autumnal diseases. The patient usually convalesces with speed, seldom requiring tonic medicines.

"This affection attacks indiscriminately both sexes, and all ages and conditions; though it is more severe upon middle aged adults, than old persons or children. The degree of danger is to be estimated by the violence of the symptoms, especially from the presence of such as denote gangrene of the stomach, or great disturbance of the brain. Though the hiccough is often a very troublesome and distressing symptom, yet it does not point out any particular danger. The first stage, seems to be a kind of chronic condition, and does not continue for any definite period. The more active forms of the complaint are very regular in duration, and seldom exceed the ninth, or stop short of the seventh day, unless in cases of fatal termination. The mortality from this disease is perhaps not so great, as might be expected from the nature of the symptoms. About one case out of twenty or thirty is lost, of those who experience the disease in its active form."

Western Quarterly Reporter.

Climate best suited for Patients afflicted with Pulmonary Consumption.—It is not a little remarkable, how many different places have been recommended as best calculated for the relief of persons labouring under pulmonary consumption; and notwithstanding each recommendation has been enforced as grounded upon ample experience, yet that the chosen spot for so desirable an end, is the subject of still greater difference of opinion at this present moment, than at any former time. At one period, it was very generally admitted that Madeira held out the greatest hopes to the unfortunate sufferer; but these hopes were blighted by the work of Dr. Gourley, an old and experienced physician, who, from a series of well observed facts, amply showed, that little relief was afforded by a residence in that island, and that even the natives themselves were rapidly, and in great numbers cut off by that very insidious disease. Madeira is now, we believe, scarcely ever thought of as a suitable situation for the phthisical invalid; the same remark may be made concerning Montpelier, once so renowned; and, in short, the whole climate of Italy has recently been assailed, as furnishing little or no benefit to consumptive persons. A part of this censure must be deemed unjust; the sufferer has doubtless often failed of enjoying the blessings of a benign climate, because of some peculiarities of constitution, or of the advanced stage of the disease. There can be little doubt that there are districts of country in the American States, as well qualified to benefit the consumptive, as any of the situations in the European world; and were this belief more general, we should not so often hear of the sad consequences that have resulted to many of those who have exiled themselves from their friends and home, and encountered the fatigues of long journeys, in quest of advantages which they never realized.

These few reflections suggested themselves from a perusal of a valuable paper on the Climate of Savannah, as a place of residence for consumptive patients, lately addressed to the Editor of the Philadelphia Journal of the Medical and Physical Sciences, and written by Dr. *William C. Daniel.* This paper we shall here insert entire.—Ed.

" I have for some time designed addressing you upon the subject of sending persons with pulmonic affections to pass the winter months in the South. My observations have satisfied me, that the instructions usually given are not the most judicious, nor best calculated to realize the views of the physician, or fulfil the hopes of the invalid.

" The doctrines I believe generally taught in the schools of the North, to be—that while a southern climate is to be sought, patients are particularly directed to avoid the *brackish air* of Savannah, and such situations as Augusta and Columbia are recommended in preference. This, previous to my residence here, was my own opinion; but all that I have seen has convinced me that I was mistaken, and I am now fully satisfied that this place has decided advantages for consumptive patients over any other in the Southern states, either more removed from the sea-board, or more immediately exposed to the sea air. It is alike exempted from the keen mountain air of the interior, and the sudden changes and dampness of the sea shore. The winds coming from the mountains are broken, and in a great degree devested of their bleak, chilling character, by their passing over our fine barrens, which extend one hundred and fifty miles to the west and northwest—and the space between this and the ocean, tempers very much, and deprives of their dampness, those winds coming from the northeast and east, and southeast. Be the causes, however, what they may, I repeat, that experience has fully proved that the atmosphere of Savannah is infinitely better calculated for consumptive patients, than either that of Charleston, Columbia, or Augusta It exercises a soothing, lulling influence, upon them, which all who have breathed it acknowledge. I have very frequently been called to visit such patients from the North, on their way to Augusta, or some other interior position, and I have seen the same persons return to Savannah with very little, if any alteration, of their symptoms, who upon remaining here for a few weeks have exhibited the most remarkable amelioration of condition. I have again seen others who had travelled to the south by land, returning home in the spring by this place, and who after an abode here of a few weeks, have professed to feel themselves much improved. It appears almost unnecessary that I should say. I consider such relief usually as only temporary—temporary, however, as in most cases it may be, it is of great importance to the sufferers—and where we cannot relieve, it is most desirable to respite suffering. But I do believe that much more advantage, and frequently of a more permanent character, would be derived, if the visit to our climate were made at an earlier stage of the disease than it commonly is ; and I am also persuaded that many lives might be saved, if such patients could be prevailed upon to pass the summer and fall months with us as well as the winter. The fear, however, of an autumnal fever, prevents this ; which fever, if I may place faith in the opinion of many respectable medical men, and in my own limited experience, usually effects a cure, by transferring (as we suppose) diseased action from the lungs to the liver. I have seen several cases in which an attack of bilious fever restored to health persons obviously in the first stage of consumption; and in such, the debilitated state of the system, and the abstemiousness of their habits, forbid a degree of fever sufficient to place their lives in serious jeopardy. I am, however, going beyond the limits I had prescribed to myself. Yet, permit me further to remark on this point, that I have known several persons in the first stages of consumption, leave this place in a condition much improved from that with which they came, who have died the succeeding summer in the middle or northern states. What would have been the influence of *our* summer upon them? It differs very materially from *yours*—and since it is capable of imparting so much robustness and vigour to invalids from our fevers, may it not produce an excitement in consumptive patients after a southern winter highly injurious to them? These, however, are questions which extensive observation can alone answer.

" The most remarkable influence which the air of this place appears to exercise, is in giving repose to the patient. I have known them reach here harassed almost to death, with restless, sleepless nights, and upon the night succeeding to their arrival, they have enjoyed refreshing slumbers—and from that time many of their most distressing symptoms have been astonishingly alleviated. This was particularly the case with a young gentleman of New-York, who being considered upon the verge of a consumption, came to pass his winter with us. A residence here that season, improved him much more than did the previous one passed upon the borders of the Mediterranean. He left us under favourable promises of a recovery, and to realize which, he was induced to make a voyage to India, from whence he returned in the following February in a very emaciated condition. Such were his sufferings in New-York, that he was induced to sail, almost immediately after his arrival from India, to this place. He informed me that his cough was exceedingly troublesome and harrassing. He had considerable fever, with profuse night sweats, which were very debilitating. He said, that for six weeks previous to my seeing him, he had not enjoyed more than two hours of repose in the twenty-four. On the night of his arrival he rested well, and his perspiration was inconsiderable compared with what it had been usually before—his cough had evidently abated, and he already felt much more comfortable. In a few days his appetite returned; his sleep, which was extended to the usual healthy term, was refreshing; his perspiration subsided, and his cough ceased to be troublesome. His improvement advanced so rapidly, that he was very soon quite a different man; and in a few weeks after his arrival, he sailed for Charleston upon a visit of pleasure. The influence, however, of the air of that place was such as to occasion his speedy return to Savannah. On his arrival here, he informed me that he ceased to feel so well soon after his leaving us, and that in Charleston he experienced the same inability to sleep, the same profuse perspiration, and the same troublesome cough, which had affected him previous to his coming from New-York. The symptoms again subsided as suddenly as upon his previous visit; and during a residence here of some months his improvement was constant and rapid. He entered into society, and partook liberally of its pleasures and enjoyments, without inconvenience. He sailed for New-York in the summer of 1820, since which time I have not heard of him. He left us under strong hopes that his disease was subdued; which, however, I did not feel myself authorized to sanction.

"This is undoubtedly an extreme case, and it is not just to suppose that others would derive a similar benefit, under similar circumstances, from a visit to this place. Yet I feel no hesitation in saying, (and I do it upon the authority of many sufferers,) that the atmosphere of Savannah is better calculated for consumptive patients, and those disposed to pulmonic complaints, than that of any position in the southern states of which I have any knowledge."

Savannah, May 22, 1822.

Letter addressed to the Editors of the New-York Medical and Physical Journal. By Samuel L. Mitchell, M. D. LL. D. Professor, &c.

New-York, Oct. 12, 1822.

Our collection at the College has been latterly enlarged by the following specimens and communications:

1. The natural productions, consisting of articles in Icthyology, Ornithology, Entomology, and various other departments of the sciences, collected by Alexander Vache, Esq. during a voyage in

the United States Ship Franklin, to Valparaiso, in Chili, and back to New-York, by the way of Cape Horn, in the United States Ship Constellation.

2. A collection of plants, in the form of a Herbarium, brought from East Florida, where they grew, by Charles Vignoles, Esq. who gathered them.

3. A parcel of specimens from Choco, and its neighbourhood, in the new Republic of Colombia, South-America, brought by Col. Woodbine. They consist of the vegetables reputed to be most efficacious against poisons and venomous bites ; of the different sorts of woods, useful for timber and in the arts, entirely different from the species growing in our forests ; and of a variety of other rare and curious things. This gentleman convinced me by his maps and expositions, that the intercourse between the Atlantic and Pacific Oceans, could be accomplished through the route summarily sketched through the Viceroyalty of New-Grenada, by Baron Humboldt. Col. W. has examined, on the spot, the tract of country which Baron H. only knew by description ; and the report is conclusive in favour of an easy passage, where the Andes seem to lower their heads for the purpose.

4. William Zollickoffer, M. D. of Fredericktown, Md. has sent his treatise on the *Prussiate of Iron*, (or Prussian Blue,) which he strongly recommends as a remedy for intermitting and remitting fevers, in doses of 4 grains to 6, twice or thrice in 24 hours. He thinks it preferable to *arsenic*, in any of its forms, and even extols it above the *Peruvian Barks*. Dr. Z. sums up the excellencies of this medicine, in these words : " The prussiate of iron possesses the following advantages over the Cinchona officinalis, as a remedy in intermitting and remitting fevers : first, it is void of taste, and may therefore be much more readily exhibited than the pulv. cinchon. offic. which to some is extremely unpleasant : secondly, it may be given in every stage of the disease, while the administration of bark is confined to the apyrexiæ : thirdly, the dose is much smaller, being from four to six grains, twice or thrice in twenty-four hours, or at morning, and noon, and night ; while bark, to be effectual, must be given in much larger doses : fourthly, it never disagrees with the stomach, or creates nausea, even in the most irritable state of this viscus, while bark is not unfrequently rejected : fifthly, in its effects as a remedy calculated to prevent the recurrence of future paroxysms, it is more certain,

prompt, and effectual, than the justly celebrated cortex Peruvian: sixthly, and lastly, a patient treated with this article will recover from the influence of intermitting and remitting fevers, in the generality of cases, in much less time than is usual in those cases where the bark is employed. In making use of the Prussiate of iron, as a remedy in disease, care must be taken to select that which is of a very dark blue, approaching to a black, having a shining coppery fracture, and adhering firmly to the tongue.

5. Baron *Andreas Joseph De Stifft*, first physician to the emperor Francis, and president of the Medical Faculty of Vienna, has sent the third edition of the *Pharmacopœia Austriaca*, in return for a copy of the Pharmacopœia of our United States. This was done through Baron *Jos. Franc. de Jacquin*, with a most obliging letter, and our friend, the Austrian consul general at New-York. This elaborate work will furnish excellent matter for the meditated improvement of our national performance, in another edition.

6. By the annual report of the London Vaccine Institution for 1822, it appears, that the confidence of the governors and managers remains undiminished. Their words are these: "The medical council of the London Vaccine Institution admits not of any controlling or modified power of vaccination. It is as sovereign a prophylactic, as complete a preventive, as is the small-pox itself, against its own future occurrence." [p. 7—8.] In this great and glorious warfare against small-pox, too much cannot be said in praise of the Director, John Walker, M. D. and the Registrar, Andrew Johnstone, LL.D.

7. A Spanish publication of high importance, from Lima, has been brought by L. Osborne, Esq. surgeon in the U. S. navy. It is the second edition, printed in Madrid, of the *observations on the climate of Lima, and its influence on organized beings, more especially man*, by the distinguished Doctor Don Hipolito Unanue, professor of medicine in the royal university of Saint Mark, director of the medical and surgical college of Saint Fernando, honorary physician of his majesty's chamber, fellow of the royal medical academy at Madrid, and Proto-medico of Peru. The author is now the minister of finance in his regenerated country; and his book is a treasury of valuable information—4to. 1815.

8. The receipt of numerous other things; such as the *American*

Knight-fish, or EQUES LANCEOLATUS from Curracoa; *the* CAECILIA TENTACULATA, or *tail-less and eye-less snake*, from Brazil; and the SIREN of South Carolina, forwarded by Dr. Thomas of Pineville; the horn of the mountain-ram of California; all add to our stores of zoology and anatomy.

9. I began to think I had written enough for this time, when tracts from Brussels, in the Netherlands, of the important discoveries made by Professor C. S. Rafinesque, of Transylvania University, presented themselves to my eye. The disclosures in natural science are contained in a series of tracts or memoirs on various subjects, among which are these: 1. A monography on the roses of North America. 2. A description of several new species of Houstonia. 3. An account of the fossil turbinolites of Kentucky. 3. A particular history of the Bivale and other molluscas inhabiting the river Ohio, containing twelve genera and sixty-eight species; and among the rest, figures, &c. of the extensive and beautiful genus of UNIO. 4. Botanical remarks on different points of the science, scientific and practical. 5. Remarks on Polystomous and Porostomous animals, and on some errors into which authors have fallen in relation to fishes. 6. Observations on certain hybrid animals; and an enumeration of the several modes in which the stamens of plants are connected or associated. 6. He has constituted into a new or undescribed species, the MANUS, or *scaly lizard*, from Africa, which Capt. Cahoone, of the revenue cutter, gave to the writer, and the latter, in the donor's name, placed in the Cabinet of the Lyceum of Natural History.

A Description of a Muscle connected with the Eye, lately discovered by W. E. HORNER, M. D., one of the Professors of Anatomy in Philadelphia.

I HAVE lately had occasion, in a dissection of the human eye, to observe a part of its muscular apparatus, which I believe not to be generally known to Anatomists, judging from the reports of my professional brethren in this country, and from a reference to the more approved authorities among the systematic writers, as Sœmmering, Albinus, Caldani, who has quoted from the first, Bichat, Sabatier, &c., of the continent of Europe; and Munro and Bell, of Great Britain.

"The part alluded to is a small oblong muscle, found on the posterior part of the lachrymal ducts. It arises from the os unguis near its junction with the os planum, and passing forwards and outwards, terminates at the internal commissure of the eyelids, near the puncta lachrymalia. As it approaches the eyelids, it splits into two parts ; one is inserted into the upper lid, and the other into the lower ; the superior fibres of the first named part, or the upper, being blended, in some measure, with the orbicularis muscle, but the lower having fully a distinct and well-marked insertion. The muscle is about a half an inch long, and one-fourth of an inch wide, its upper and lower margins being well defined. I have also had occasion to make some new remarks on a tendinous structure enveloping the lachrymal ducts, and the position of these ducts, by which the curvature of the internal commissure of the eyelids is kept up, and which would prevent any deformity of the lids, even if the tendon of the obicularis were cut through in the operation for fistula lachrymalis. The origin of the muscle is at least half an inch posterior to the orbicularis tendon, at its attachment to the nasal process, and it lies on the tendinous matter including the ducts. To get a view of this muscle, cut through the eyelids, and separate them from the ball, except at the internal canthus ; then turn the lids over on the nose ; remove the valvula semilunaris and the tunica conjunctiva in the neighbourhood, with the fatty matter, and you will be sensible of the arrangement described. Its use is to draw in the puncta, and to keep the edges of the eyelid properly adjusted to the ball, &c."

We find the foregoing in the London Medical Repository for July, 1822. In a note, the Editor, Dr. Copland, subjoins the following explanations and opinions :

" Mr. Shaw favoured us with this communication, which was contained in a letter from Professor Horner to him. We have placed it among the *Original Communications*, because its author wished to have it announced in a British journal, previously to its appearance in a future volume of Transactions of the Philosophical Society of Philadelphia.

" Mr. Shaw has also favoured us with the following opinion respecting its originality, in which we entirely concur :—' I think that the muscle described by Professor Horner, is the same which has been already depicted in Rosenmuller's plates; but the description given here is so different, and so much better, that I have no hesitation in admitting Professor Horner's claim to the merit of having discovered it, without having derived any assistance from the labours of other Anatomists."

University of the State of New-York—College of Physicians and Surgeons of New-York.—The College of Physicians and Surgeons will commence their Course of Lectures for the ensuing Winter Session, on the first Monday of November, (the 4th,) at the College in Barclay-Street.

Dr. Post, on Anatomy and Physiology.

Dr. Hosack, on the Institutes of Medicine and Practice of Physic.

Dr. Macneven, on Chemistry.

Dr. Mitchell, on Botany and Materia Medica.

Dr. Hamersley, on the Clinical Practice of Medicine.

Dr. Mott, on Surgery.

Dr. Francis, on Obstetrics and the Diseases of Women and Children.

The BOARD OF TRUSTEES deem it proper to make it known, that in conformity with the Ordinances of the Hon. the Regents of the University, passed in March, 1822, every Student is required to attend two full Courses of all the Lectures delivered in this College, before he can be a Candidate for the Doctorate: unless said Student shall have previously attended Lectures in said College, prior to the Session of 1822—3, or shall have previously attended one entire Course of Lectures, delivered in some other respectable Medical School or University.

The Matriculation Fee is FIVE DOLLARS, which includes the Use of the Library.

N. B. Under existing circumstances, the Course of the Clinical Medicine is sub-graduate.

Dr. Thomas' Treatise on Domestic Medicine.—The venerable and experienced author of the *practice of physic*, so generally known in this country and in Europe, has lately published a work of some extent with the above title. The reputation of Dr. Thomas has been long established; and on the present occasion, we must admit that he has given a treatise on popular medicine which promises to surpass all former undertakings of a similar kind. To this American edition, the American editor has made several valuable additions, calculated to render the work still more generally useful. The additions, we are informed, have been furnished from the MS. improvements of the author himself.

Boylston Prize Questions.—The following questions have been announced for 1823.—1. On the functions of the extreme capillary vessels in health and disease.—2. What textures of the body, when punctured or lacerated, are most liable to bring on the disease called Tetanus?

The following questions for 1824 are also announced by the committee.

3. On the diseases resembling syphilis, and the best means of treating those diseases.

4. How long may the human body remain immersed in water without extinction of life? And at what period after immersion, would it be useless to employ restorative means?

Dissertations on these questions are to be transmitted to D. Townsend, M. D. of Boston, on or before the first Wednesday of April in each year.

We understand that Dr. EBERLE's work on *Therapeutics and the Materia Medica* is ready for the press, and will make its appearance in December next, in two vols. 8vo. about 440 pages each.

The editors hope to be enabled to present their readers with additional facts from Drs. Cotton and Harlow, concerning the remarkable case of extra uterine conception, published in the present number of this Journal.

FOREIGN.

Baron Larrey on Yellow Fever.—Baron Larrey has lately published a small work on the yellow fever, entitled *Considerations sur la Fievre Jaune.* Not having been so fortunate as to see the original work, we avail ourselves of the following analysis of it, contained in one of the late English Journals.

" Mr. Larrey thinks, in common with many of his countrymen, that the YELLOW FEVER consists in inflammation, of greater or less severity, of the biliary system, the intestinal canal, and the serous membranes. He considers himself justified in this opinion by the results of the examinations he has made after death ; an evidence, we think, not to be too implicitly depended upon. M. Larrey says, that the principal causes of this disease are, hot and moist climates, intemperance, spirituous liquors, &c. On the subject of contagion he holds some peculiar opinions. He divides virus into two kinds ; the one fluid, as in syphilis, small-pox, and vaccina ; the other gaseous, or miasmatic : of this last sort is the virus of the yellow fever. Each virus has a particular influence on certain parts. That of the yellow fever acts particularly on the nervous system of animal and organic life : it is connected with the lymphatic system. According to M. Larrey, it is the most subtle and fugacious of all. It lasts but a moment at the highest point of disease ; and then loses the power of transmiting itself. It is principally seated in the exanthema when this exists, and in the cutaneous transpiration. It is in this manner that M. Larrey endeavours to reconcile the contradictory opinion of physicians on this subject. He is, however, an advocate for measures of precaution ; for if, says he, one patient only out of a hundred be capable of transmitting the disease, prudence requires that the whole should be sequestered, as it is impossible to ascertain the individual by whom the disease may be perpetuated.

" The TREATMENT of the disease which M. Larrey recommends, is consistent with the view which he adopts of its nature. In the first instance, bleeding from the jugular vein, or from one or both of the temporal arteries, with the application of ice or iced water to the head, produce the best effects. The irritation and inflammation of the intestinal canal may be best combated by the application of puncture cupping to the lumbar and hypocondriac regions, in parallel lines, following the course of the nervous fluid, from the positive to the negative pole.* In the second period, M. Larrey recommends slight purgatives, e. g. calomel, from two to ten grains, according to circumstances, cold drinks, and emolients. In the third stage, slight tonics, cinchona, bitters, camphor, and neutral salts. In the last place, for the precautions necessary to the preservation of health, the Baron refers us to the work of Dr. Descourtils, entitled, "Guide Sanitaire des Voyageurs aux Colonies. 1816."—*Quarterly Journal of Foreign Medicine for April*, 1822.

Professor Osiander on Uterine Vagitus, and the respiration of the Fœtus.—"At the sitting of the Royal Society of Sciences of Gottingen, on the 11th of November, 1820, the Director, Professor Osiander, read a memoir, containing a series of observations on the vagitus, respiration, and vitality of the human fœtus, during and immediately after parturition. He has, in this paper, given the results of his experience on these points for upwards of twenty years. The principal facts are the following :

"1. The human fœtus, whether full grown or premature, can breathe and cry when the head has passed the external parts, although it be closely confined from the narrowness of the parts, and whether the delivery has been effected by the natural efforts, by the hand, or by the forceps. The author has confirmed this assertion by twelve cases, for the most part observed in the lying-in hospital.

"2. In difficult delivery, effected by the forceps, the child cries at the moment of the passage of the head, although the forceps have not been removed.

"3. In nine cases the child breathed and cried as soon as the head was born, although the funis was twisted at least twice round the neck, and in some of the cases very tightly ; in four cases the cry was heard, and the child seen to breathe immediately after the funis was disengaged.

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* *Quart. Journ. For. Med.* Oct. 1820. v. ii. p. 394.

"4. Two children of unusual size, of which one weighed ten pounds, cried as soon as born, although one had been fixed in the pelvis for two hours, from the narrowness of the genitals, and although the anterior fontinelle was perfectly ossified.

"5. Two children cried immediately after birth, although the neck was compressed by the arm and hand, the latter having been expelled with the face.

"6. *Four children* born with the membranes unbroken and transparent, *being surrounded by the liquor amnii, were seen to open and shut the mouth as a fish under water.*

"7. This phenomenon was not observed in another, born under the same circumstances.

"8. Two, on the contrary, opened the mouth, breathed, and cried, notwithstanding the face was covered with the empty membranes.

"9. One child, born, and covered wholly by the membranes, ruptured them, and drew them over its face.

"10. One child cried as soon as the head was expelled, although its mouth was filled with blood escaping from the mother.

"11. *Some children delivered by the feet breathed and cried, although the head was still in the uterus.*

"12. In one instance, the waters having escaped, and the pains being very severe, *the child was heard to cry in the uterus so distinctly, that many students present authenticated the fact by their signature in the note-book of the Institution.* The crying was heard under the same circumstances, in his private practice, by Professor Osiander, by the mother, and the standers by.

"13. A dropsical child, extracted with much difficulty, gave signs of life.

"14. A dead fœtus, affected with anasarca and ascites, according to the mother, moved distinctly before birth.

"15. A child died during labour, and was extracted quite dead. Although no air had been inflated, the lungs were found considerably distended, and floated in water, a proof that respiration had been perfectly performed in the uterus before birth.

"16. A deformed child, like a dead fœtus, breathed after birth with a sensible noise, and died in a few moments. On dissection no traces of air were found in the lungs, although they were very expansible.

" 17. A child, prematurely born, had the skin of an orange colour, and the cuticle was detached in many parts ; it lived, sucked, and left the institution alive at the same time with the mother.

" 18. A blind woman was prematurely delivered from the effects of fright. The child breathed freely during delivery but died in a few moments : the lungs sunk completely in water.

" 19. In a child born dying, blood flowed from the umbilical vein, although the circulation had ceased in the umbilical artery.

" 20. A child was extracted with the placenta, the cord being neither cut nor tied. Pulsation was observed for ten minutes in the umbilical arteries, but it was not until the child had freely breathed that the dark blue of the blood was changed into a bright red colour.

" 21. The heart and carotids pulsated for a length of time without respiration, but there was no pulsation in the umbilical arteries. Blood flowed when the jugulars were opened, but it was impossible to restore the child to perfect life, although powerful contractions were excited in the muscles for many hours, by the galvanic pile ; the other signs of life were wholly lost.

" 22. A child had advanced very slowly during labour, and at last seemed altogether fixed. On examination, the very extraordinary observation was made, that the child held the umbilical cord firmly with its hand, so that it was necessary to open the fingers to set this part at liberty. At the institution of Gottingen, where it is customary to weigh all the children that are born, it is not uncommon to see some of them clasp the cords of the scales so firmly, that it is necessary to employ some force to oblige them to quit their hold. Should we any longer wonder that Hercules strangled serpents in his cradle ?"—*Quarterly Journal of Foreign Medicine, for April*, 1822.

Dr. Richardson on the Climate of Egypt and Nubia in Pulmonic Diseases.—Nothing can exceed the beauty of the mornings and evenings in Nubia. The air is light and clear, and cool, and all the senses, as if bathed in the breath of heaven, cling with rapture to every blade of grass, or every opening plant. During the whole of yesterday the sky was without a cloud, and in the evening the constellations were particularly bright. All of us had seen the sky of Italy and Greece, but for brightness, the nocturnal sky of Egypt and Nubia surpasses them all, as much as they do that of England. Many a pulmonic patient, who feels his sickly fabric chilled and pierced by the snow-winds of Vesuvius, would be soothed and healed by the unirritating and balmy air of Egypt and Nubia. Nor is the distance so apalling; it only seems far to those who have not tried it. The traveller will go in a shorter time, and with less fatigue, from Marseilles to Alexandria in Egypt, than at an average rate of travelling, he can go by land from Geneva to Naples. The accommodation is always good, and there is no chance of incurring fresh exacerbations of disease; all the way up the Nile he carries his house along with him, leaves it, and returns to it when he pleases. His mind is constantly engaged with the unremitting succession of new and interesting objects that occur in every step of his journey, and that without one single circumstance to discompose or annoy him; every where, he can purchase, at a moderate rate, such articles of provision as are necessary for his comfort, excepting wine, and that he can easily carry with him from Alexandria and Cairo. His medicines he ought to take from London.—*Travels in Egypt and Palestine, by Dr. Richardson, vol.* i. *p.* 392.

Black Urine.—It appears from Dr. MARCET's paper in the Medico-Chirurgical Transactions, that he has met with some cases in which black urine had been voided. At the request of Dr. Marcet, some was examined by Dr. PROUT, who gives the following account of its chemical properties:

The residuum obtained from this urine by evaporation, not only does not contain any lithic acid, as was observed by Dr. Marcet, but no urea can be detected in it by the tests which indicates its presence.

Although the addition of dilute acids produced no immediate change of colour in the urine, yet, on standing for some time, a black precipitate slowly subsided, leaving the supernatant fluid transparent, and but slightly coloured.

The black precipitate thus obtained, was found to be nearly insoluble, either in water or alcohol, whether hot or cold. It readily dissolved in cold concentrated sulphuric and nitric acid, forming a deep brownish-black solution; but, on diluting the acids with water, the black substance appeared to be again precipitated unaltered. These acids, however, by the assistance of heat, apparently decomposed it. The black substance readily dissolved in the fixed alkalies, and in the alkaline subcarbonates, forming very dark solutions. The addition of water did not affect these solutions; but acids re-precipitated the substance apparently unchanged. When ammonia was employed as the solvent, and the excess expelled by evaporation to dryness, a black or deep brown residuum was obtained, which appeared to be a compound of the black substance with ammonia, and possessed the following properties:

It was very soluble in water; and, on being heated with caustic potash, it gave off the smell of ammonia. The black compound, however, did not appear to have any tendency to assume the crystalline form.

In evaporating to dryness, on a piece of glass, the ammonical solution in which the black substance had been dissolved, the residuum split into most minute fragments, having a regular and very peculiar appearance, especially when examined with a magnifier.

From the solutions of this compound in water, muriate of barytes and nitrate of silver produced copious brown precipitates, as did also protronitrate of mercury and nitrate of lead; but oxymuriate of mercury produced no immediate precipitate, and that obtained from acetate of zinc was of a paler brown colour.

From these experiments, Dr. Prout concludes that the remarkable specimen of urine in question owes its black colour to a compound of a peculiar principle with ammonia, as Dr. Marcet had inferred from his own trials; but he is, moreover, inclined to think that the black principle itself, such as is obtained from the urine by the action of dilute acids, may be considered as a new body possessed of acid properties. From the small quantity of the specimen, however, which could be spared for Dr. Prout's experiments, it was impossible to obtain complete and decisive evidence on the nature of this substance; but it appears to be sufficiently characterized as a peculiar acid, and to bear a closer analogy to the lithic acid, or rather to some of the compounds which it forms when acted upon by the nitric acid, than to any other principle usually found in the urine.

Should this view of the subject be confirmed by further observations, Dr. Prout would propose to distinguish this new substance, on account of its black colour, by the name of *Melanic acid.*—(*Annals of Phil.*)

M. J. A. Lejumeau de Kergaradec has lately published a small memoir upon the application of Auscultation to the Study of Pregnancy. By means of the *stethoscope* or *pectoriloque* of Laennec, he is of opinion, that the pulsation of the heart of the fœtus may be distinguished from those of the arteries of the mother; and that, by an attention to the different sounds communicated to the ear, we may determine, in doubtful cases, whether the fœtus in utero is alive or dead. MM. Kergaradec and Laennec are both of opinion, that the *stethoscope*, and the immediate application of the ear, are attended with the same advantages; but M. Fodera considers, that although, for the purposes of delicacy, the *stethoscope* may be somewhat preferred, yet that we may frequently be able to detect diseases by the immediate application of the ear, which we are not able to do with the *stethoscope*; and he consequently gives the preference to auscultation performed in that manner, in all cases where the *stethoscope* may be considered necessary.—*London Medical Repository, for August*, 1822.

Instances of Children born without the Œsophagus.—A male child was born in August, 1820, at the full period of utero-gestation. It was of the full size, and took the breast with avidity; but each time the milk was immediately returned by the nose and mouth, with rattling in the throat, and symptoms of impending suffocation. It, however, lived in this state for eight days. During this time, the alvine and urinary secretions were regularly voided, although in smaller quantities. Derangement about the œsophagus was suspected, and in order to ascertain its nature, an inspection was instituted after death. *Dissection.*—The liver was very large, and reached rather below the umbilicus, but its form and colour were natural. The stomach was without a cardiac orifice, and at this part was united to the diaphragm by means of cellular tissue. The lung on the right side was distended, and presented a reddish colour; that on the left was compact, and dark in its appearance, evincing that the child had only breathed with the right lung. The heart possessed its usual characters at that age. The œsophagus was entirely wanting, and the pharynx formed a cul-de-sac at its inferior part.—*Hufeland's Journal*, 1821.

An instance entirely similar to the one just related occurred sometime since, to Dr. Hallam, of Walworth. This child lived six or seven days. During that time, whatever was attempted to be given was returned in the manner already described. The alvine and urinary secretions were regularly voided, but in smaller quantity. On inspection, after the child's death, the œsophagus was found wanting, as in the case recorded by Hufeland. The pharynx and stomach, with the intervening parts, were preserved by Mr. Hallam, and are in the possession of Sir Astley Cooper.

These cases are interesting, chiefly on account of the regular appearance of alvine evacuations which occurred in them both. This circumstance furnishes a very conclusive proof, that at least the chief part of the intestinal excretions consists of secretions from the mucous surface of the digestive tube, especially from the follicular apparatus, and of the excrementitious parts of the hepatic and pancreatic secretions.—*Lond. Med. Rep. for Aug.* 1822.

Excision of Bronchocele, by Mons. Dupuytren, of Paris.—A female applied at the Hôtel Dieu in Paris, for the removal of a large bronchocele, by which she had long been distressed. Her employment was rural, her temperament bilio-sanguineous, and constitution sound. Eight years previously, a tumour, large as a filbert, was perceived on the anterior and middle portion of her throat. During six years its growth was constant, but gradual and slow; in the seventh, it rapidly increased. Its progress now was rapid; it covered the whole front of the neck, and had three distinct lobes. The central one only was painful; it extended over the sternum, four inches in diameter.

A Parisian surgeon removed the diseased parts. No hæmorrhage followed; and at the end of a month the wound cicatrized.

About six months afterwards, the two lateral lobes acquired an enormous size; the central also enlarged; deglutition was embarrassed, and the respiration rendered laborious and afflictive. Urged by these circumstances, and the unhappiness arising from a sense of deformity, she declared to the surgeons her fixed resolution of submitting to excision of the tumour, whatever should be the result. Drs. Pelletan and Dupuytren gave their explicit opinion that such an operation was altogether impracticable. Nevertheless, she returned in a few days, and entreated its performance. It was again declined; and the danger to which it would expose her described in the strongest terms. Palliatives were prescribed; but neither apprehension of pain nor of danger, nor formal refusal, could alter her purpose. Displeased and desperate, she appeared a third time, and pressed her original request. Such great perseverance at last excited a deep interest in her behalf; and she was admitted, on January 1, 1808, into the hospital, where the state of her neck was again minutely examined.

The anterior and lateral regions of the throat were occupied by a tumour, stretching perpendicularly from the inferior maxilla to the sternum and clavicles, and transversely from the one maxillary angle to that of the opposite side. It measured seven inches in length, and the same in breadth, and consisted of three soft and knobbed lobes. The central was least protuberant; it adhered to, and moved stimultaneously with, the larynx. The lateral lobes were attached to the contiguous parts, and admitted motion in all directions. The integuments were flaccid; the jugular veins, and their ramifications, much dilated; the pulsations of the superior thyroid arteries, above the tumour, were violent and perceptible; the carotids throbbed with great activity and distinctness. Pain was not felt in the tumour; but it sensibly impeded respiration, obstructed deglutition, and, when the patient experienced lively emotions, produced irregularity of the cerebral circulation. Her face, on such occasions, assumed a deep red hue; and dazzling of the eyes, with vertigo, supervened.

The recent progress of this dreadful affection, with the sense of imminent suffocation experienced by the patient, formed certain indications of a fatal issue. On the other hand, the situation, size, and connexions of the tumour, displayed the danger of attempting its excision. After mature deliberation, therefore, it was once more pronounced impracticable. This declaration overwhelmed the unfortunate sufferer; she was seized with gloomy despair—with profound melancholy; she adopted the resolution of accelerating death by inanition, and firmly rejected all kinds of food. Her menstrual discharge, hitherto regular, now became suppressed. Violent spasms succeeded, with a fearful sense of suffocation, convulsions, horror, and excruciating pains.

Immediate dissolution, in these circumstances, was inevitable. By the operation, she might possibly be saved. It was promised; and her mind became calm and animated. Her sound constitution, strength, youth, and earnest desire for relief—the great mobility of the tumour, its slight union with the integuments, and the knowledge of a partial extirpation of the parts having already been successfully made—all presented themselves as motives of hope to M. Dupuytren, and determined him to undertake it. Accordingly, with the assistance of M. Rullier and others, it was performed, in the presence of Professor Pelletan and many surgeons of the French metropolis.

Operation.—The integuments of the anterior and middle parts of the neck being elevated, so as to form a transverse fold, they were divided perpendicularly in the centre down to its base. The incision was then prolonged upwards to the symphysis of the chin, and downwards to the superior border of the sternum. The skin of the left half of the tumour was now detached by careful dissection. In the course of this, two sets of veins were discovered; one lying close to the tumour, the other subcutaneous. Most of these were avoided; the rest were tied, and divided between the ligatures. On the left and posterior side of the tumour, four thyroid arteries, greatly enlarged, were easily recognized. Being denuded and doubly secured, they were divided between the two ligatures. In this manner the left half of the tumour was detached, with no other inconvenience to the patient than the pain induced by a protracted dissection.

The right lobe was then separated, with equal precaution. Its connexion with the circumjacent parts readily yielded to the finger and handle of the bistoury. Without difficulty, also, the internal jugular veins, carotid arteries, and pneumo-gastric nerves, were detected, and pushed aside from the way of the knife. Now M. Dupuytren ascertained the practicability of removing the whole mass. For this purpose the lateral portions of the tumour, which had been separated from their attachments, were turned up, and drawn forward so as to stretch the central lobe, which adhered closely to the larynx and trachea. The cellular substance, which connected it to these parts, was now divided with the knife, when the wind-pipe came into view : its surface was flattened by the pressure upon it by the incumbent tumour. Lint was placed in the bottom of the wound, its lips approximated, a light dressing superadded, and the ends of the ligatures left in its inferior angle.

The patient endured this tedious and intricate operation with fortitude. She lost only a few spoonfuls of blood. Once or twice she was threatened with syncope, and was sick. After the operation, her face was pale, its expression altered ; her strength, in every respect, overpowered ; the pulse was frequent, small, and concentrated ; the respiration laborious and rapid ; the skin, cold. Cardialgia and constant nausea were established ; and she appeared overpowered by the mere effect of the operation. To recruit and support her strength now became the prominent indication. Cordials were administered in small proportions. She was threatened with suffocation, on taking even a spoonful of liquid ; and this could only be done by her when in a vertical posture. Towards the evening, however, her symptoms began to improve. Prostration of strength was now succeeded by re-action ; the pulse was frequent and full ; respiration nearly natural ; the countenance flushed ; the skin hot and dry ; and some spoonfuls of fluid were introduced into her stomach, without inconvenience. But the hopes thus awakened were of short duration. By the evening of next day, her breathing became painful and stertorous ; and she died.—*London Repository*, March, 1822.

Taddei's New Antidote to Corrosive Sublimate.—Professor Taddei, of Florence, has published an 8vo pamphlet, proposing the gluten of wheat as a new antidote for that dreadful poison, the oxymuriate of mercury. We have not yet seen it, but there is a very full analysis of it by A. Cattaneo, in Omodei's Annals, minutely describing the origin of the discovery ; the mode of preparing and administering the gluten, in which there seems to be nothing peculiar ; experiments with it on animals ; and a comparison of its efficacy with that of albumen, as proposed by Orfila. Taddei concludes his pamphlet with recommending the now almost disused corrosive sublimate in syphilis, care being taken to modify its effects by drinking largely of milk, barley water, and decoction of mallows, or burdock.—*Annali. Univ. di Medicina*, vol. xvii.

METEOROLOGICAL OBSERVATIONS

FOR JUNE, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer.			Winds.			Rain Gauge.	Hygrometer.*	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 A M	2 PM	9 P M			
1	30 20	30 20	30 16	68	84	70	s	s	s		772	cloudy
2	30 10	30 10	30 10	73	90	79	se	sw	sw		784	do
3	30 10	30 10	30 10	77	88	74	nw	sw	sw		720	clear
4	30 10	30 10	30 18	70	91	65	sw	sw	ne	—.24	760	cloudy
5	30 29	30 32	30 30	61	74	64	ne	se	se		648	do
6	30 32	30 30	30 30	64	73	64	ne	ne	ne		643	do
7	30 27	30 18	30 08	65	74	68	sw	sw	sw		643	variable
8	29 90	29 90	30 04	71	85	74	sw	sw	sw		620	cloudy
9	30 04	30 04	30 04	70	89	75	nw	sw	sw		595	clear
10	30 06	30 06	30 06	70	93	75	w	w	w		572	do
11	30 00	30 00	30 00	73	88	72	w	sw	sw	—.15	600	variable
12	29 97	29 97	30 10	73	93	73	n	n	n		584	clear
13	30 25	30 30	30 34	65	80	70	n	s	s		580	do
14	30 48	30 48	30 50	65	70	60	se	se	se		575	cloudy
15	30 50	30 50	30 25	65	85	75	se	sw	sw		584	do
16	30 02	30 00	30 00	70	89	72	sw	sw	nw		572	clear
17	30 00	30 00	30 00	70	88	72	nw	nw	nw		573	cloudy
18	30 00	30 00	30 00	64	72	64	nw	nw	sw		573	do
19	30 12	30 12	30 12	62	80	64	sw	sw	sw		586	clear
20	30 18	29 94	29 94	64	65	64	sw	se	se	1. 01	608	cloudy
21	29 94	29 90	29 90	64	80	70	s	s	s		616	do
22	29 90	30 00	30 08	70	85	72	nw	w	w		600	clear
23	30 18	30 18	30 18	70	84	71	w	sw	sw		594	do
24	30 18	30 18	30 18	72	85	75	sw	sw	sw	—.22	630	cloudy
25	30 18	30 18	30 18	70	74	67	ne	sw	sw	—.19	664	do
26	30 18	30 10	30 10	68	81	68	e	e	e	—.04	700	do
27	30 18	30 20	30 20	65	84	72	ne	ne	sw		696	variable
28	30 20	30 20	30 20	64	88	75	sw	sw	sw		628	clear
29	30 20	30 20	30 20	72	92	78	sw	sw	sw		628	do
30	30 20	30 20	30 20	75	92	78	sw	sw	sw		639	cloudy

The quantity of rain which fell this month, is 1 inch and 85—100ths.

At the commencement of this month the atmosphere was very humid, and the mornings foggy; there was but little thunder and rain. On the 2d and 21st, there fell a small quantity of rain from Nimbus passing over. The sun was obscured on the 20th and 25th, and the greatest part of the 1st and 8th; the sky generally was clear at night; those days on which there was but little wind the heat was very oppressive, especially on the 10th and 11th. Vespertine lightning on the 2d, 16th, and 30th.

* This is a very neat instrument, invented by Capt. Kater, in which the space between extreme moisture and extreme dryness is divided into 1200 equal parts.

METEOROLOGICAL OBSERVATIONS

FOR JULY, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer.			Winds.			Rain	Hygrometer.	Weather.
	7 A M	2 P M	9 P M	7 A M	2 P M	9 P M	7 A M	2 P M	9 P M	Gauge.		
1	30 18	30 18	30 18	78	94	73	sw	sw	s	—.16	653	cloudy
2	30 18	30 12	30 10	75	87	71	s	sw	sw	1. 63	685	do
3	30 08	30 00	30 00	76	87	76	sw	sw	s		700	do
4	30 00	30 08	30 18	76	93	75	s	s	s		686	do
5	30 20	30 24	30 20	75	88	76	s	s	s		694	do
6	30 20	30 20	30 20	77	87	75	s	s	s	1. 84	720	do
7	30 20	30 10	30 10	78	92	75	n	s	s	—.18	736	do
8	30 04	29 94	29 90	75	92	74	sw	sw	sw		748	do
9	29 90	30 04	30 18	76	85	76	nw	nw	nw		660	clear
10	30 33	30 33	30 30	73	91	80	nw	sw	sw		653	do
11	30 30	30 22	30 16	77	85	76	sw	sw	sw		650	variable
12	30 08	30 08	30 08	75	86	74	sw	sw	nw	—.34	710	cloudy
13	30 10	30 10	30 00	76	84	72	nw	nw	nw	—.67	726	do
14	30 00	30 00	30 00	72	88	78	sw	sw	sw		732	variable
15	29 96	29 98	30 06	78	87	72	sw	w	w	—.11	730	cloudy
16	30 10	30 18	30 24	72	88	76	w	nw	nw		700	variable
17	30 30	30 30	30 30	70	87	77	nw	nw	sw		660	clear
18	30 30	30 30	30 30	75	92	79	sw	sw	n	—.06	663	variable
19	30 30	30 26	30 20	76	92	78	n	s	s		665	do
20	30 18	30 10	30 10	79	96	78	sw	sw	sw	—.04	656	cloudy
21	30 18	30 18	30 18	79	90	78	sw	sw	sw		645	clear
22	30 10	30 06	30 00	76	85	78	w	w	sw		645	do
23	30 00	30 00	30 00	76	90	76	sw	sw	sw	—.28	675	do
24	30 00	30 00	30 00	78	88	71	sw	n	n		655	cloudy
25	29 94	29 98	30 00	72	82	76	w	w	w		665	do
26	30 08	30 20	30 26	68	80	70	w	nw	nw		630	clear
27	30 28	30 28	30 28	65	81	73	nw	nw	nw	—.32	645	clear
28	30 20	30 12	30 12	70	88	75	s	s	nw	—.03	670	cloudy
29	30 10	30 26	30 26	73	83	73	nw	n	n		650	clear
30	30 40	30 40	30 40	70	85	74	n	e	e		634	do
31	30 47	30 47	30 40	71	87	70	e	e	e		650	do

The quantity of rain which fell this month, is 5 inches 66—100ths.

This month was remarkable for its unusual humidity, and its numerous and heavy falls of rain. We were visited by thunder, lightning, and rain, on the 1st, 2d, 6th, 7th, 18th, 20th, 24th, and 28th. Vespertine lightning on the 3d, 7th, and 28th. The sun was totally obscure on the 6th. The rain on the 27th. fell in the night.

METEOROLOGICAL OBSERVATIONS
FOR AUGUST, 1822.
Made at the New-York Hospital, by Richard Pennell. M. D.

—	Barometer.			Thermometer.			Winds.			Rain Gauge.	Hygrometer.	Weather.
	7 AM	2 PM	9 PM	7 AM	2 PM	9 PM	7 AM	2 PM	9 PM			
1	30 40	30 40	30 38	70	84	72	e	e	sw		656	clear
2	33 30	30 22	30 20	71	87	73	sw	sw	sw	—.27	670	cloudy
3	30 16	30 16	30 10	75	89	76	sw	s	s		705	do
4	30 03	30 00	29 80	76	94	74	s	s	nw	—.42	698	do
5	29 90	30 06	30 10	74	80	70	nw	nw	nw		665	clear
6	30 10	30 22	30 28	64	78	67	ne	ne	ne		647	do
7	30 38	30 40	30 40	67	75	68	e	se	se		643	do
8	30 47	30 47	30 47	64	82	72	se	ne	ne		638	do
9	30 47	30 40	30 40	72	86	70	sw	s	s		645	do
10	30 40	30 40	30 34	72	88	72	s	s	s		650	do
11	30 30	30 30	30 30	72	92	74	sw	sw	sw		645	do
12	30 30	30 28	30 23	74	90	79	sw	sw	sw		648	do
13	30 23	30 20	30 20	75	89	79	sw	sw	sw		652	do
14	30 32	30 32	30 22	70	85	70	nw	nw	nw		670	cloudy
15	30 10	30 00	30 00	74	92	75	s	s	s	—.02	662	do
16	30 17	30 17	30 17	70	90	76	nw	sw	sw		650	clear
17	30 17	30 17	30 17	74	91	78	sw	sw	sw		664	do
18	30 20	30 20	30 20	70	86	74	ne	ne	ne		680	cloudy
19	30 15	30 15	30 00	72	85	75	se	s	s	—.17	700	do
20	30 00	30 00	30 00	77	94	76	s	s	s		660	do
21	30 00	30 00	30 00	75	88	75	nw	nw	nw		660	do
22	30 08	30 10	30 18	73	85	74	e	e	e	—.17	660	variable
23	30 25	30 25	30 25	65	80	71	e	ne	ne		672	cloudy
24	30 25	30 25	30 25	69	80	75	se	se	s	—.03	670	do
25	30 36	30 36	30 36	66	85	74	s	ne	se		660	clear
26	30 36	30 36	30 30	70	84	70	se	se	se		660	do
27	30 30	30 30	30 20	68	78	68	ne	ne	ne		656	do
28	30 16	30 16	30 16	67	86	0	ne	ne	ne		660	cloudy
29	30 20	30 20	30 20	72	88	7[illegible]	ne	ne	ne		660	clear
30	30 40	30 40	30 40	64	81	69	ne	ne	ne		656	cloudy
31	30 40	30 40	30 40	65	82	70	ne	ne	ne		656	do

The quantity of rain which fell this month, is 1 inch and 8—100ths.

This month attracts our notice for the number of devastating tornadoes that happened in different parts of the United States; its arid atmosphere, clear starry nights, and moderate rains. 4th. Clear morning, wind S.; at 3 P. M., wind changed to N. W.; it soon began to blow tremendously; the dust ascended in large quantities, so as to obscure some parts of the city; it had prostrate a number of trees, and did considerable damage; it was accompanied with loud thunder, lightning, and rain, which lasted until half past 4 P. M. We had likewise thunder, &c. on the 15th and 22d. A beautiful double rainbow was seen in the south on the 15th, at 7 P. M. Vespertine lightning on the 4th, 15th, 19th, and 20th. On the night of the 9th and 10th, I observed a number of shooting meteors.

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CASE I.

ART. I. CASE *of* OSTEO SARCOMA—*in which the right side of the lower jaw was removed successfully after tying the* CAROTID ARTERY. By VALENTINE MOTT, M. D. Professor of Surgery in the University of New-York.

CATHARINE BUCKLEW, the subject of the following operation, was an interesting young woman, aged about seventeen years, of a healthy appearance and good constitution.

She says that about two years since, a swelling commenced behind the last molar tooth of the lower jaw, attended with acute pain about the angle of the jaw, that continued about three weeks; at which time it left her without any evident resolution of the inflammation. At this period there was no inflammation of the integuments, nor could any pus be discovered either on the cheek or about the bone within the mouth. Some domestic applications were made to the cheek, but the tumefaction continued to increase, and assumed a smooth, hard, and bony character.

About twelve months after its commencement she applied to a physician in New-Jersey, who advised her to apply blisters to the cheek, and the use of topical applications of caustic to the tumour, together with a general antiphlogistic constitutional treatment. After having submitted to this course for two months without experiencing any benefit, she came to this city, and became my patient.

The first molar tooth came away early in the disease, and the second soon followed; then, three or four of the other teeth of that side of the lower jaw. She states, that previously to this disease she had never had a decayed tooth.

No fluctuation was to be felt at any time in the tumour. She had no constitutional symptoms as the effect of this disease, nor any inordinate headache on that side. The lymphatic glands of the neck were however swollen, during the continuance of the inflammation in the early part of the disease; but they disappeared as soon as the pain subsided.

When she came under my care, the tumour extended from the root of the coronoid process to the second bicuspid tooth, elevated nearly an inch above the level of the teeth, and spreading considerably wider than the alveolar process. Its appearance was smooth, and to the touch somewhat elastic, though firm. An incision on each side of the alveolar margin, with a scalpel, enabled me pretty readily to remove the tumour with a gum-lancet to the level of the jaw-bone. The tumour, on examination, contained many cartilaginous and osseous spiculæ, and in the substance of it was imbedded one of the molar teeth in a perfectly sound state.

About three weeks after this operation a small portion, of the size of a nutmeg, which had granulated and grown rapidly, was taken off, and soon after she retired to the country, and remained in a very comfortable state for several months. The tumour began now to re-appear, and continued to increase gradually in every direction.

The tumour at present, (Nov. 10th, 1821,) has the same firm and slightly elastic feel which characterized it in the early stage, involving all the right side of the inferior maxillary bone. Projecting outwards, it produces great convexity of the cheek; upwards it divides into two portions, the outer and longest reaches up to the os malæ, and between the two is a considerable furrow, formed by the teeth of the upper jaw, which occasions an abrasion and constant discharge; the latter though offensive, does not appear to be acrid or irritating: downwards it comes nearly in contact with the thyroid cartilage; inwards it extends beyond the middle line of the mouth, pushing the tongue and uvula very much to the left side, having the velum pendulum palati of the right side attached to it in its whole course. The inward portion is considerably raised above the level of the tongue when the mouth is opened.

The posterior extremity of the tumour has encroached so much upon the passage leading into the posterior fauces, and the pressure of the lower parts upon the larynx is so considerable, as to render deglutition very difficult; and from the great difficulty of mastication, she has been compelled for some time to subsist upon liquid aliment. Her speech is considerably interfered with in consequence of the displacement of the tongue. She experiences no pain in any part of the tumour.

The gradual increase of the disease rendering mastication and deglutition more difficult and distressing, she is very desirous of knowing if an operation could not be performed which might extend to her some chance of life; observing, that with the constant growth of the tumour, such as has taken place for a few weeks past, she should not be able to swallow any thing in a short time. Fully aware of the dangerous nature of the novel operation her case requires, she is determined to submit to it, and hazard the consequences; the uncertain result of which I carefully explained to her, and informed her, that she might die during the performance of the operation; but that I believed it to be both practicable and proper.

After preparing the system for about a week with light diet, and the exhibition of several doses of neutral salts, to obviate any great degree of inflammation, the operation was commenced about 11 o'clock on the morning of the 17th.

As most of the important branches of the external carotid artery would be interfered with in the course of this operation, I believed it most prudent to pass a ligature around the trunk of the carotids as a first and preparatory step. This would not only enable me to go through it with more safety to the patient. but appeared the most important of all means to avoid inflammation. Indeed inflammation was much to be dreaded, from the immense extent of the external incision, and the violence which would necessarily be done to the tongue, palate, and pharynx.

From these considerations, I felt it doubly important to intercept the current of blood through the common carotid, and from what I had observed to attend the application of ligatures to the large arteries of the extremities, in cases of severe injuries, by preventing inflammation, I thought great advantage would attend it in this case, as I am satisfied will be fully shown.

An incision about two inches and a half long was made a little below the thyroid cartilage on the inner edge of the sterno-cleido-mastoideus muscle, and after exposing the carotid, a single ligature was passed under it and tied. In this situation it was deemed most proper to tie the carotid in order to prevent the second part of the operation from interfering with the first incision. Very little blood was lost, only one small cutaneous branch at the lower angle of the wound required a ligature; yet she became pale and almost pulseless during, and immediately after, the operation, notwithstanding her position was recumbent. She submitted to the operation with great firmness and resolution, but her mind soon became agitated and perturbed to a great degree, and it seemed altogether impossible for her to regain her former fortitude. The operation was suspended, and some cordial was administered, but it failed to remove from her mind the presentiment that any further proceeding at present would be fatal. In this state of remarkable agitation I resolved not to proceed, and informed her that with such fears as she then entertained, the result was to be dreaded. The wound was then dressed, and she was put in bed, faint and exhausted.

After recovering a little, I apprised her that this was only preparatory to the most important part of the operation, and that what had been done would prove of little or no benefit to the disease, and urged her seriously to consider of it, and if possible make up her mind to submit to the performance of the remaining part, which should by no means be deferred longer than the following day.

One o'clock, P. M.—She is still pale, and in a cold sweat; pulse has not recovered itself; and when asked, nodded that she felt some uneasiness.

Seven o'clock, P. M.—Much more collected; pulse natural; no uneasiness whatever, except some obtuse pain about the wound in breathing, and in swallowing saliva; no increase of heat; left a student to watch with her through the night, and again took leave, earnestly recommending to her private consideration the expediency of submitting to the remainder of the operation.

18th.—Seven o'clock, A. M.—Found her this morning in a very composed state of mind; having slept well, and free from fever. Upon putting the question, would she submit to the remainder of the operation? she nodded assent with much apparent decision, and said she was determined to undergo it.

At ten o'clock, finding my patient cheerful and resolute, she was again placed upon the table, and, in the presence of William Anderson, surgeon, Dr. Hosack, and a number of other gentlemen, the operation was continued. Feeling for the condyloid process, an incision was commenced upon it, opposite the lobe of the ear, carried downwards over the angle of the jaw in a semicircular direction along the lower part of the tumour, as it rested upon the thyroid cartilage, and terminated it about half an inch beyond the angle of the mouth, on the chin. The termination of this incision upon the chin, was just above the attachment of the under lip to the bone, and the mouth was thereby laid open. I now extracted the second incisor tooth of that side, as it was in a sound part of the bone, and, after separating the soft parts from the side of the chin, and laying bare the bone, I introduced a narrow saw.

about three inches long, similar to a key-hole saw, from within the mouth, through the wound, and sawed through the jaw-bone from above downwards. The lower part of the tumour was then laid bare, by cutting through the mylo-hyoid muscle, and the flap of the cheek carefully separated and turned up over the eye. This exposed fully to view the whole extent of the tumour as it rose upwards to the os malæ. After the integuments were carefully dissected from the parotid gland, the masseter muscle was detached from its insertion, until it came to the edge of this gland, then separating a thin plane of the fibres of this muscle, I now readily raised the parotid, without wounding it at this part. The maxilla inferior was now laid bare just below its division into two processes, and it appeared sound. To facilitate the sawing of the bones, it was necessary to make a second incision, about an inch long, close to the lobe of the ear, and terminating at the edge of the mastoid muscle; then, with a fine saw, made for the purpose, smaller and more convex than Hey's, I began to saw through the bone, obliquely downwards and backwards, and finished with one less convex. The latter part of the sawing was done with great caution, to avoid excruciating pain from the laceration of the inferior maxillary nerve. When the bone was sawed through, the two processes were observed to be split asunder, and the coronoid to be drawn up by the action of the temporal muscle.

An elevator was now introduced where the bone was divided at the chin, by which the diseased portion was raised, when, with a scalpel passed into the mouth, the tumour was separated from the side of the tongue, as far back as the posterior fauces, from the velum pendulum palati and pterygoid processes. This loosened it very much, so that it could be turned upon the side of the neck. It was then separated from the parts below the base of the jaw, and also from the pharynx, and detached at the posterior angle, carefully avoiding the trunk of the internal carotid and deep-seated jugular vein, both of which were exposed.

The diseased mass, being now separated above and below, was turned up, the pterygoid muscles detached, and the third branch of the fifth pair of nerves divided from below, a little above the foramen at which it enters the bone. By this manner of proceeding, with a constant reference to this nerve, I apprehend my patient was saved from much acute pain, and the nerve more safely divided, than at an earlier stage of the operation.

At several periods of this operation, the curved spatulas, used in my operation upon the *arteria innominata*, were found very useful, particularly in elevating the parotid gland, and keeping the tongue steady, whilst the tumour was separated from it.

Very little blood was lost during this operation. Two arteries only of any size were divided, the facial and lingual; and these only required the ligatures at the branch extremities; but each end was tied for safety. Another small artery behind, and a little underneath the posterior angle of the jaw, yielded some blood and was tied.

The flap of the cheek was now brought down, after waiting a few minutes to observe if any hemorrhage should come on, and secured in close apposition by three sutures, and adhesive straps. Lint, a compress, and the double-headed roller, completed the dressing. She was made as comfortable as possible upon the table, and directed to remain a few hours to recruit, and to be more convenient in case any hemorrhage should make it necessary to remove the dressings.

At eight o'clock in the evening, I found her removed to a bed, and in a comfortable situation. Some reaction of the circulation had taken place, but there had been no hemorrhage. The pain from the operation, she said, was less than she expected. For the first time, since the operation, she sipped three tea-spoon's full of cold water, and gave evidence, by a nod, that she could swallow. Directed one hundred drops of tinct. opii to be given, if any twitching, more pain, or restlessness, should supervene.

19th.—Seven o'clock, A. M.—Found her quite free from fever and irritation, and, in every respect comfortable. Swallows cold water by the tea-spoon full with but little inconvenience. Did not take the tinct. opii last night. Slept several hours during the night.

Twelve o'clock, at noon.—Is comfortable; skin moist; pulse less frequent, and soft; directed an enema to be administered of soft-soap and water; has a little more difficulty in swallowing, but none in breathing.

Nine o clock, P. M.—As well as in the morning. Enema operated three times, and relieved her. Pulse frequent, but not tense. She has taken about two ounces of cold water by the tea-spoon full since daylight.

20th.—Seven o'clock, A. M.—Had a very comfortable night. This morning, instead of nodding she answers "yes" and "no" to the several questions in an audible whisper.

Nine o'clock, P. M,—Much as in the morning.

21st.—Nine o'clock, A. M.—As comfortable as yesterday morning.

Nine o'clock, P. M.—No material alteration.

22d.—Nine o'clock, A. M.—Directed an enema to be administered as before. Allowed her to take, in addition to her cold water and teas, some thin chicken soup: is in every respect doing well.

Nine o'clock, P. M.—Tumefaction of the lips and cheek very trifling, not enough to effect the least change in the eyelids of the right eye.

23d.—Is in every respect comfortable.

24th.—Eleven o'clock, A. M.—Makes no complaint; dressed the wounds; union by adhesion has taken place in the whole extent, excepting about the ligatures and sutures. Suppuration having come on about two of the sutures, they were removed. Pulse about 120. Renewed the adhesive straps with lint interposed between them and the wound, and the double-headed roller.

25th.—Every way comfortable. Pulse 120.

26th.—Says she has no complaint to make. Pulse 80. Directed her to take a small dose of sulphate of magnesia.

27th.—Speaks audibly, and says she is very well. Pulse about 84.

28th.—As well as before; dressed the wounds; removed the two sutures at the upper part near the ear; wounds appear healed at every part, except where the ligatures remain upon the arteries. Pulse 80.

29th.—Feels very well; speaks distinctly; takes freely of soup and other thin food: pulse 100.

Dec. 3d.—Ligature from the carotid came away, and the other three ligatures from the upper wound. A small collection of matter was evacuated from under the integuments in the lower wound, which was produced by the irritation of the ligature.

4th.—Speaks and swallows very well; wounds just healed. Has used for some days a wash of spirits and water to the mouth, with a view to correct some fœtor of the saliva, and cleanse the mouth.

6th.—Found her dressed and sitting in an adjoining room, reading by the fire; looks and says she is very well. The bandages being all left off, the only deformity apparent is a little more tumefaction of the right cheek than the left; wounds just well; can move very readily the sound half of the under jaw. Permitted her to chew some animal food.

10th.—Wounds all healed—makes no complaint.

March, 1822.—To-day having visited her, I found scarcely any perceptible deformity. The right cheek appeared, upon close examination, to be a little more depressed than the left. I felt from within the mouth some osseous deposit to have commenced at the two situations at which the bone was divided. Her health in every respect is perfectly good, and she enjoys the free use of the left side of the lower jaw.

Nov. 5.—I have repeatedly heard of and seen the patient during the past season, and she continues to enjoy uninterrupted health.

25 Park-Place.

EXPLANATION OF THE PLATES.

No. I.

This drawing exhibits the appearance of the face before the operation was performed. The rotundity of the right cheek will be observable, and the integuments below the under eyelid, on the stretch from the size of the osteo sarcoma.

No. II.

Represents the side of the face after the tumour was removed, and before the parts were closed.

a The part at which the jaw bone was divided near the chin.
b Condyloid process, where separated by the saw; the coronoid process being drawn up by the temporal muscle.
c The side of the tongue.
d Sublingual gland.
e Molar teeth of the right superior maxilla.
f Inferior portion of the parotid gland, with a plane of fibres of the masseter muscle turned up.
g g Portions of the common integuments.

No. III.

This figure exposes the appearance of the tumour upon its inner surface.

a Cuspidatus and first bicuspis teeth.
b Lower jaw where divided anteriorly.
c Bone at its posterior division.
d Furrow produced by the teeth of the upper jaw.
e e Two superior portions of the tumour covered by the membrane of the mouth.
f f The remainder of the inner surface of the tumour, dissected from the parts within the mouth.

No. IV.

The appearance of the patient after her recovery.

ART. II. OBSERVATIONS *on the Climate and Diseases of the Island of* CURACAO. *By* DANIEL L. M. PEIXOTTO, M. D. of New-York.

No writings of physicians, whether ancient or modern, have commanded more general attention, or secured more lasting regard, than those descriptive of the connexion existing between climate and disease. The cause of the value of such observations is obvious. They trace the most extensive and fruitful sources of disease, point out the most effectual methods of mitigating their force, or, perhaps, of wholly preventing their agency, and direct the choice of those who are forming new settlements, by warning them against such spots as experience has discovered to be unhealthy, and designating the signs of those that are friendly to human health and longevity. It were a needless task to cite the names of the authors who have distinguished themselves in this department, from the days of Hippocrates down to our own times, when a Chalmers, a Cleghorn, and a Fergusson, claim our admiration and respect.

Following, at an humble distance, the example of the great writers to whom I have alluded, I am induced to throw together a few observations collected during my residence in the island of Curacao. The facts they present will not be uninteresting, inasmuch as they relate to a place with which our commercial intercourse is frequent, and of which but little or no notice has hitherto been taken in a medical point of view; and, further, as they confirm various principles which have, of late years, deservedly attracted a great share of attention.

I know not how it has happened that the slight mention made of Curacao, by one or two authors of works on tropical climates, should convey the idea of its being a very unhealthy place, and of its climate being one of those distinguished for their unfriendliness to Europeans. That the island has, at times, been visited by epidemics, and been temporarily converted into an abode of disease, (and where is the spot, the most healthy, that has not been thus afflicted ?) that northern subjects on their first arrival in it, are exposed to those inconveniences which result partly from the change of climate, partly from the tropical situation of the place, I am not disposed to deny ; but that it should be cited in conjunction with those places in the West Indies, whose localities, abstractedly considered, render them permanently unhealthy, neither experience will warrant. nor my regard for truth, suffer me to pass by without contradiction. Thus Lind, in one sweeping paragraph, denounces " the Dutch settlements at Surinam. St. Eustatia, and Curacao, as all being very unhealthy ;"* and in a page immediately succeeding, he observes, " the Havana is not quite so unhealthy as Carthagena or Curacao." How an author, professedly writing a correct description of the different places in the West Indies, could have permitted himself to confound the dry, rocky, and proverbially healthy islands of St. Eustatia and Curacao, with the marshy situations of Carthagena, Havana, and, above all, with that of Surinam ; the atmosphere of which is eternally moist, appears to me most surprising. Again, Moseley, in condemning the authority of Rouppe, a Dutch physician, whose means of information appear to have been limited, asserts of Curacao, that " its harbour is surrounded with marshes and stagnant waters."† This fact, we must presume, he borrowed from Rouppe himself, who had charge of a ship's crew at Curacao for two months, during which time they suffered much by disease. But the affections under which they laboured, of which Rouppe's description may be found in a note to Lind's work‡, do not seem to have been other than such as are indigenous to tropical latitudes, generally developed by causes existing on shipboard. In proof of this view, it will be seen by an examination of the description referred to, that the sickness prevalent on board of Rouppe's ship had made its appearance previously to her arrival at the harbour of Curacao. "Die decimo nono portam Curacao intravimus, et *tunc* viginti ægrotaverunt." But even if the disease had broken out after the ship's arrival at the Island, as this event took place in August, which month, as well as September, is remarkable throughout the West India Islands for the prevalence of long continued calms, was there any coeval cause necessary to excite disease in newly arrived Europeans, confined on board of vessels of war, and that eighty years ago, when the subject of the health of seamen was imperfectly understood and little attended to? But, the cause, as has been shown, was altogether attributable to the general action of a hot climate, and not to any deleterious miasm arising from "marshes and stagnant waters." Indeed, there is not a swamp to be found in the whole island, nor any stagnant water, even in rainy seasons, for several miles from the harbour; the shores of which are chiefly table-rock, washed by the tide of the ocean, and this circumstance moreover obviates any objection arising from the consideration that places are known to undergo many variations in their localities in a period of years. I have been the more particular on this subject as it has a close relation to a principle of great application in the investigation of tropical diseases.

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* Lind on Hot Climates, page 86.

† Moseley on Tropical Diseases, p. 129—fourth edition.

‡ Lind on Hot Climates page 93.

Curacao is situated in latitude 12° 8′ N. and longitude 69° W. distant twenty leagues from the coast of South America, of which the highlands are plainly discernible in clear and calm weather. The island extends from the N. W. to S. E. in length forty miles, and its mean breadth may be fifteen. The general appearance of the island is barren and parched, its surface broken into abrupt masses of hill, which raise their picturesque forms in every direction. They are, however, bare of all marks of vegetation or verdure; cultivation being confined to the vales in which the fruit and maize plantations are more advantageously located.

The soil is superficial, consisting of a light mould covering a rocky substratum, in which gypsum, pumice, and ochre, and some argillaceous earth are found. Water is to be had by digging into the rock; but there are no rivulets of fresh water, nor any marshes to be seen in any part of the island. The hills are of no inconsiderable height, some of them having an elevation of from ten to fifteen hundred feet above the level of the sea. They no doubt serve a highly useful purpose, by collecting and conducting the currents of air along the valleys; and adding to their force and impetus, they refresh the atmosphere. In one of these hills on the northern part of the island, there is a highly interesting cave, comprising several spacious and romantic apartments and vaults, from the walls and roofs of which the most beautiful stalactites are pendent, while in the floored recesses there is water of the purest kind.

The air is pure and dry; seldom, or never, darkened with those mists and fogs which enwrap us so frequently further to the north. Clouds are constantly driven over the island by the brisk trade winds that prevail throughout the year, excepting in August and September. They are highly favourable to the reduction of the extreme solar heat, by the constant succession of grateful shade which they lend to the earth.

It is a circumstance worthy of note, that fires are with difficulty lighted in this island, and unless assiduously fed, are instantaneously extinguished. Hence the carelessness of stupid negroes, who expose lighted coals near to plank floors, the frequent, and often lamentable source, in other islands, of the most ruinous conflagrations, escapes here with impunity. To what causes can this be owing?

Metals oxidate rapidly after exposure. The most polished steel soon loses its lustre, and assumes the dingy hue of rust. What circumstances they are that occasion this in an atmosphere so dry as that of Curacao, does not at first clearly appear. Cleghorn* observed the same phenomenon at Minorca, and imputes it to the moisture arising from the spray of the sea, that in windy weather is driven over the whole island, as evinced by the brine found on the leaves of vegetables growing in parts the most remote from the ocean. Our more sagacious countryman, Chalmers, assigns as a cause for the rapid oxidation of metals in Charleston, South Carolina, the existence of an acid in the atmosphere exhaled from the surface of the rich and moist earth.* But it does not appear that his view of the origin or nature of this acid was very clear or satisfactory. That moisture is not sufficient to produce this effect must be evident; for if it were, the oxidation of polished metals should be rapid in direct proportion to the dampness of the soil and the humidity of the atmosphere: but this is by no means the case. Bontius† asserts, that at Java metals acquire rust sooner in the driest season of the year than they do in the dampest seasons in any part of Europe. This conforms with my observations at Curacao; in which the air is constantly dry, and the rust is acquired by metals much sooner than it is in New York in our most wet seasons. I should therefore be inclined, with Chalmers, to call in the aid of an acid not arising from the soil, but furnished by the decomposition of the muriate of soda, disseminated either by means of the spray of the sea, as is believed by some, or as has been maintained by others, by exhalations from the sea itself. That the atmosphere in the neighbourhood of the sea is impregnated with salt, does not admit of a doubt.‡

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* Diseases of Minorca, p. 1.

The constant evaporation by heat of the salt thus carried into the atmosphere precipitates, according to an experiment by J. Murray of London, free muriatic acid. If this interesting position be admitted, we have a satisfactory solution of the difficulty, and the rationale of the oxidation of metals in dry situations near the sea, is not problematical. But whether this solution be admitted or not, the acid principle evidently acquired from the sea must still be resorted to. May not this acid have an agency in the rapid extinction of fires before described?

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* Chalmers on Diseases of South Carolina, v i. p. 11.

† Moseley, p. 100. Note.

‡ For some highly interesting facts on this subject, I cannot do better than refer to a paper on Salt Storms, published by my friend, Dr. John B. Beck, in Silliman's Journal, vol. i. No. 2.

The temperature, as observed by Fahrenheit's thermometer, in the shade, ranges in the course of the year from 76 to 90; the ordinary heat being 85. The seasons differ less in this, than in the larger islands to the north, neither do the rains recur at regular periods as in most other tropical situations, being frequently wanting altogether for a succession of seasons. This was especially the case for the years 1820, 21. During all this period there has been scarcely any benefit of rain felt, except from a few occasional showers, and occurring at protracted intervals, and consequently, a total failure of the crops has been experienced, to the great loss of the planters. With the exception of a few fruit trees, to which drought is favourable, there was no appearance of vegetation any where to be seen. The leaflless desolation of winter reigned under a tropical sun; ordinary wells were exhausted; animate and inanimate nature suffered under the burden, and seemed ready to waste away and perish. Domestic animals, particularly, suffered for the want of herbage and water. In one plantation six hundred out of one thousand sheep perished. Nevertheless the inhabitants experienced little or no inconvenience in their persons, neither was there any increase of mortality, or sickness, observable, not even among strangers who visited the island. To the constant perflation by the N. E. winds, which, in these years, were scarcely observed to cease even in the calm months, as well as to the absence of "marshes and stagnant waters," that would elsewhere, during the process of evaporation and drying, have exhaled the most deleterious effluvia, must be, in the main, attributed this escape from disease. The same immunity of the inhabitants of places suffering under great drought has been frequently observed elsewhere. Among other instances, Chalmers records, that during the year 1752, the inhabitants of Charleston suffered dreadfully from oppressive heat and drought, but never was there a more healthy season known, "so long as the weather continued steadily warm and fair." These facts accord with a law laid down by that faithful and accurate observer Rush, which is, that the same "state of the atmosphere, whether cold or warm, moist or dry, continued for a long time without any material changes, is always healthy."*

Hurricanes are very rare in this island. Since the severe one of the 17th of October, 1807, there has been none experienced.

The city of Curacao is built on the S. E. part of the island, on a narrow slip of land, washed on one side by the sea, and on the other by the arm of a bay, which forms the harbour, and stretches far inland. It is surrounded by a wall and bastions on three sides, while its commodious quay is open to the harbour in front. The streets are paved, and, with the exception of one, are narrow. The houses are spacious and lofty, and thicky piled upon each other. I have been induced to ascribe to this circumstance of the streets being narrow, the superior healthiness of the crowded population. The height of the houses, and their proximity to each other, prevent the sun's rays from heating the circumambient air to that oppressive degree, which is so insupportable in places in which wider streets and more ample spaces give free scope to the torrid sun to exert his energies in full play. The narrow streets, moreover, attract the breezes, and direct them, with additional impulse, upon the verandas fronting each house.

The rest of the island is composed of the suburbs of the city, and of plantations. In these, Guinea corn and the ordinary tropical fruits grow in the highest perfection. The medlar, the mango, orange, lime, fig, banana, tamarind, and cocoa, are among the principal of the many and delicious fruits that in gay confusion enrich this barren soil, and administer to the simple luxury of the inhabitants.

There is nothing very particularly remarkable in the manners of the people. They are in general characterized for sobriety and early hours, and they live to the same good old age which is enjoyed in the most favoured regions of the globe. The females menstruate early, and there are not wanting several instances of the menses not ceasing until after fifty. The secluded habits of this sex, the relaxing influence of the climate, and, among the coloured portion, (of course the most numerous,) the too early indulgence in sensuality, predispose them to chlorosis, amenorrhœa and leucorrhœa, diseases which are very common, and frequently resist all ordinary methods of cure. They become mothers early, and bear pregnancy and parturition with an ease and nonchalance that would half justify Byron's exclamation:

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* Rush's Account of the Climate of Pennsylvania.

> "Who round the North for paler dames would seek?
> "How poor their forms appear, how languid, wan, and weak!"

It has already been observed that Curacao is proverbial for health. The absence of deleterious causes of disease, the pure and dry air, the old age to which the inhabitants reach, all testify to this fact. To it invalids from the neighbouring islands frequently resort, and with decided benefit. Convalescents from the endemic fevers of St. Domingo and Jamaica find immediate relief on their arrival in Curacao; and the most obstinate cases of tertian and remittent fevers, here frequently yield to the salutary impressions of a healthful air. Even broken-down constitutions, the sure, and, in hot climates, speedy attendants on dissipated habits, are here refitted, or, at least, enabled to bear "life's burden" with comparative ease.

Phthisical patients are, however, seldom benefited by a stay at this island. The dry constitution of the air seems to be rather injurious to those labouring under pulmonary affections. The acidity, too, with which we have supposed this atmosphere to be impregnated, adds no doubt to this injurious disposition. The asthmatic likewise find this place unkindly to their affections. Of this I had proofs in my own case. When a child, I laboured under this complaint here, and it was not until I changed climate that I found relief. On my revisit to the island of late I had a recurrence of the complaint, and suffered several violent paroxysms.

Among the endemial diseases of this place may be first mentioned those affecting the eye. Weak sight is a very general complaint among the inhabitants, even in those that are young;

and hence the use of glasses is very common. The more serious affections of this organ to be met with are ophthalmies, pterygia, cataracts, and amaurosis.

The causes of the prevalence of these disorders are not unworthy our notice. The rays of a vertical sun darting on a barren soil, over which verdure seldom or never throws those refreshing hues on which the eye delights, in more fertile regions, to repose, are reflected with a dazzling intensity which proves irritably painful to the eye; and inducing in that delicate organ an indirect debility, powerfully predisposes it to be excited to disease. So the sterile plains of Egypt, for the same reason, have long been noted for their unfriendliness to the vision of the natives. But in Curacao the white lime, with which all the buildings, both public and private, were formerly washed, has been a more actively exciting cause of these disorders. On this account it was that the government issued a decree forbidding houses to be covered with any colour lighter than yellow; since the operation of which the inhabitants are said to suffer less. The same cause has been observed to produce similar effects at Bologna, in Italy, and at the isle of Malta.* If it were necessary to enter more into detail, the neighbourhood of the sea, the occasional prevalence of measles, and, in former years, of the small-pox, might be cited as additional causes of the many defects and diseases of the eye prevalent in this place.

Hernias are frequent, particularly among the negroes, whose skill in reducing them is very great. Fistulæ, particularly in ano, subsequent to piles, are often met with, and seem to be connected with the bilious disposition so incidental to those living in warm latitudes.

I could not discover that any case of hydrophobia had ever occurred here. The disease is generally known by name only.

In particular months of the year the inhabitants are exposed to be poisoned by certain fishes; more particularly the yellow bill sprat. This poison does not appear to be a permanent property of the fish, as they are eaten with impunity in general throughout the year, and it is at times only that they become possessed of this deadly venom. Whoever becomes the unfortunate victim of the unsuspected poison, is either hurried off by immediate and excruciating convulsions, or he suffers and lingers with violent symptoms of a bilious cholera, till death comes to his relief. Sometimes the poison is eliminated and counteracted by means of emetics and subsequent stimulants; but the very few cases that fell under my own observation proved speedily fatal.

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* Assalini on the Plague, p. 126.

The measles occasionally reign here epidemically, and they commit great ravages. In the early part of 1821 they made their appearance, and continued until the middle of June, proving very fatal, particularly to children. They were in most cases attended with inflammatory symptoms, and the secondary fever frequently assumed a malignant type, or went off with a diarrhœa, which proved in the sequel obstinate and difficult of cure. The fatality of this disease was owing, in most cases, to a neglect of the antiphlogistic regimen and treatment; alexipharmics and confinement being frequently resorted to in consequence of the ignorant fears of the blacks, and the far more unpardonable ignorance of the empirics. On the other hand, early depletion by the lancet, or, where the case was less violent, cathartics, and a strict conformance to rules of abstinence, and the free admission of cool air, were found successful in allaying the violence of the disease, and preventing those chronic sequelæ, usually incident to this complaint. Candour, however, requires the confession, that the same treatment in various members of the same family was not always attended with desired success, and that where no visible difference in the cause or symptoms of the disease could account for such variety of issue. The epidemic was observed by the physicians generally to assume its most violent aspect some time after its first appearance, and then gradually to abate until its final extinction.

The dysentery may next be mentioned, and as occurring more frequently among the blacks. I did not observe any symptom, as rendering it worthy of particular description. The acute form of this disease, however, frequently ended in chronic diarrhœa, which, unless properly managed, frequently wastes the strength of the patient, and finally destroys him. The treatment from which I experienced the greatest, and often most decided benefit, was to deplete or vomit the patient in the earlier stages, according to the degree of inflammatory symptoms present, and the condition of the stomach, as indicated by the tongue, &c. After this, my dependence throughout the disease was placed upon calomel, combined with rhubarb or opium, according to circumstances. A mild laxative, as castor oil, was occasionally interposed, and with much benefit. If the disease had assumed its chronic form, I added the infusion of some light bitters, among which camomile was the most grateful. In the leeward harbours of the South American coast, this excellent tonic is deemed a specific in the treatment of the disease, particularly when combined with milk.

I shall next treat of fevers, premising a few general remarks that will apply in common to all of them.

It has been too much the fashion to refer to the exclusive agency of bile, every disease which flourishes in hot weather and climates, and, under this impression, to include them all under the broad and vague appellation of bilious. It was thus formerly with putrefaction, with which all tropical diseases were necessarily supposed to be connected, and hence the term putrid, superadded to their frequently unmeaning list of titles. Moseley has sufficiently exposed the fallacy of this doctrine, and shown the dangerous tendency it produced on supposed and gratuitous curative indications. The error, as regards bile, is no less glaring, and no less productive of danger. The indiscriminate use of remedies calculated to act on the liver, to the absolute neglect of the more prompt means requisite to arrest the progress and allay the violence of inflammatory action in more vital organs, (to which affections robust, plethoric new-comers, are most subject on their *first* arrival,) has been the frequent and fatal effect of this unphilosophical spirit of generalization applied to the diseases of warm latitudes.

Bilious symptoms are always present, to a greater or less degree, in the fevers of the tropics. The sympathy subsisting between the liver and skin, induces in the functions of the former, a similarity of affection with that, which those of the latter never fail to undergo, under the operation of continued and high degrees of heat. Hence this important viscus is seldom or never in a perfectly sound state, even in those who are in seeming possession of health. But no sooner does the frame succumb to disease, be it of whatever kind, than the biliary organ is among the first to experience the effect, and, in many cases, it forms the most prominent point of attack.* This is more especially the case in the ordinary fevers of the climate to which the epithet bilious, is, in general, applied with strict propriety. But frequently these morbid affections are alleged as causes, or as constituting the diagnostic symptoms of diseases in which they are, in fact, no more than accessary or incidental occurrences. Thus yellow fever, it has been maintained, is a bilious disorder, because bilious symptoms are occasionally present during the disease, and post-mortem examinations generally show some affection of the liver to have preceded death. But, from what has been said, it must appear that the conclusion drawn from these circumstances occurring in yellow fever, must, to say the least of it, involve no small share of doubt. When we further consider that in this disease other organs invariably present a far greater degree of lesion, both in the symptoms of the disease and on dissection, this doubt will amount to a certainty that the yellow fever is not a bilious disease.

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* In hot climates a sound state of the liver is never to be expected after death, whether the disease has been acute or chronical; of the latter class of disease it is almost always the seat and origin.—MOSELEY, p. 443.

The bilious fevers, then, are those in which the liver is the origin of the disease, and bears the onus of suffering in common with the viscera subservient to it, as evinced by the most marked and uniform symptoms of the disease. The obvious indications, and the method of cure, which, when early attempted, is generally successful, consisting of emetics, cathartics, diaphoretics, and mercury, all tend to the same conclusion. The creoles and the seasoned Europeans and Americans are equally obnoxious to their attacks; those being especially their prey who indulge in an abuse of the non-naturals. Many pay a dear annual tribute to these fevers; frequently making hair-breadth escapes, from the shock of which they no sooner recover, than they undauntedly resume their habitual course, till it at last brings them to the goal of both their pleasures and their pains. These subjects generally carry with them some mark by which they may be distinguished, a chronic ulcer, the piles, fistula, or, to top the climax by the severest of all ills to 'which flesh is heir to,' dyspepsia, and, its faithful Achates, hypochondriasis.

The natives are more particularly subject to catarrhal, nervous, and typhoid fevers. I had no opportunity of seeing these, except as they occurred in solitary instances, and these were mostly cases of children, in whom the causes of disease were those indigenous to all places in every climate.

Nervous fevers are, however, as I am informed, epidemical at times, especially after a season of rain, followed by intense heat and calms, when many of those, who, during the reign of the ordinary healthy seasons, were accustomed, with impunity, to indulge ad libitum, now pay the forfeit of their sins against the laws of temperance and sobriety. In the cure of these fevers, after properly premised evacuations, the bark and snake-root are early admissible.

Intermittents are seldom seen in Curacao. The observation made by Hillary on Barbadoes will apply to this island with strict accuracy. "Intermitting fevers," observes that author, "especially tertians and quartans, are very rarely or never seen in this island, unless they are brought from some of the leeward islands, or some other places less cultivated, and not cleared of wood."* A similar observation on Savanna la Mar, in Jamaica, was made by Jackson.† In Curacao they are occasionally brought from the States, and from the large islands and harbours on the leeward coasts, and, as I before remarked, they are speedily subdued. But although the intermittent is not native here in general, exceptions do occur, and some have fallen under my observation. I attended, at different periods, two negroes from the plantations, who were affected, the one with a quotidian, the other with a tertian type of intermittent fever. These negroes, as I was correctly informed, had been engaged in labour in low places, near to which I found, on examination, ponds of stagnant water had been temporarily formed by partial, but, as is usual within the tropics, heavy showers of rain. This fact strikingly illustrates a principle formerly not well understood, that places which are in general remarkable for their salubrity may, by the operation of transient causes, change their character, and become, for a season, unhealthy. Thus situations that are ordinarily dry may, after the formation of heavy rains, or by the overflowing of rivers of fresh water, of pools, standing waters, or temporary marshes, change features and localities with those that are habitually moist; while these latter, by the agency of the same causes, may become, for a while, free from their usual causes of disease. The observation made by Volney in Egypt, that "a boundless plain, at different seasons, is an ocean of fresh water, a miry morass, a verdant field, or a dusty desert," beautifully exemplifies the gradual change which the same district of land undergoes in its aspect and medico-topographical character, and accounts for the varying degrees of healthiness and unhealthiness that give rise to opposite conclusions drawn from observations made at different periods.

To return. I could not entertain a doubt as to the peculiar cause that excited the fever in the cases which I have recorded, because there was no other case of the disease in the island; and secondly, because a removal of my patients to the dry sea-shore lent almost instantaneous efficacy to the ordinary medicines employed to arrest the disease.

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* Rush's Hillary, page 21.

† Jackson on Fevers, page 5.

It has been questioned whether typhus fever can or does prevail excepting in cold weather. That warm weather is equally or more favourable to its production and extension, cannot, however, be doubted, after the mass of testimony extant in medical records, if we begin with Pringle and continue our examination through the later and more able authors who have written on typhus since his time. I shall not dwell on this point, but proceed to observe, that if it were true that hot weather was a certain preventive of this fever, it should follow, as a regular consequence, that it could never prevail in the torrid zone. Is this, however, the fact? If we examine the writings of Thomas, Mc Arthur, Ferguson, and all English physicians who must be supposed to have been well acquainted with the characteristics of typhus, we shall find that they have given their unequivocal testimony in favour of the prevalence of this disease in the West Indies. Their testimony on this subject is the more invaluable, as they do not appear to have been biassed by any theory in favour of either side of the disputed question; of which dispute it is more than probable they did not dream. The following brief account will tend to show that it can originate under the fervor of the vertical sun, and be imported into an island suffering under heat rendered more intense by a long state of drought.

In July, 1821, an extensive emigration took place from the coasts of Venezuela. The success of the Colombian forces compelled those Spaniards, who were unwilling to abjure their faith to the mother country, to desert their homes at a moment's warning, and, embarking in crowds on board of the first vessels they could reach, to disperse themselves over the neighbouring islands. On board of these vessels they were, of course, ill accommodated; and during their passage they suffered immeasurably from the privations to which they were necessarily subjected, in the absence of every thing like comfort or cleanliness, and, above all, from the depression of spirits, which their situation and the gloomy prospects before them could not fail to inspire. Nearly two thousand of these arrived at Curacao, where many were compelled to crowd together in small habitations, destitute, in many instances, of every convenience. Many of these emigrants likewise suffered much by the change of water, which was, at this time, owing to the long continued drought, both scarce and not always of the best quality. They had not been long in the island before typhus fever appeared here and there among them, and it proved in some instances very fatal, but in the greater part less violent and more amenable to the aid of medicine. I observed that most of those who were attacked had come from places on the main, in which the remittent and typhus were endemic, as Porto Cabello, and places in the interior, &c. and I was, on account of this circumstance, led to conclude, that the subjects of the disease must have received the seeds of it before their departure from their homes, and that the depressing influence of the calamities of war, with the other exciting causes of low fever to which they were subsequently exposed, had proved the means of developing the disease. In these cases, the same loss of sensorial power and depression of nervous energy, which is observed in the typhus of authors, the same pungent heat and invincible stricture on the surface; the same quick, low, and irritable pulse; the black and parched tongue, tremulous too withal on exposure; the sordes about the gums and lips; the dull eye; the locked up state of the secretions and excretions; the exacerbation at night; and towards the close of the disease, the low delirium and subsultus tendinum; fœtid and involuntary discharges, petechiæ, &c. were to be observed; and when thus combined, ended in the death of the patient from the eighth to the eleventh day. In cases where the disease terminated favourably, its course was protracted for several weeks. Dysenteric symptoms were occasionally joined to the fever, and in many instances pulmonary irritation was very constant, and added to the restless torments of the sufferer. The treat ment was such as has elsewhere been resorted to in typhus gravior, making allowance for the climate and the peculiar condition of the sufferers. Bleeding was in no instance resorted to, but copious evacuations from the bowels, gentle diaphoretics, and blisters, in the early stages, followed in the further progress of the disease, by the use of stimulants and opiates, according to circumstances, attention to the alvine discharges not being omitted, were ever found highly beneficial. On the other hand, a blind neglect of the evacuating system, and the too early recourse had to stimulants, by untimely fears of debility and putrefaction, never failed of doing harm, and generally betrayed itself by the subsequent more direful symptoms. I cannot omit adding, that contagion did not in the least appear to be a property of the fever, all those affected having been equally subjected to the same causes, and in no instance was the disease communicated to others. So that after a short period it ceased altogether.

The last disease of which I shall have occasion to speak, is the yellow fever. To pass over in silence a subject so interesting to his country would, in an American physician, be as culpable, as it would be unpardonable, on the other hand, to tire his reader with a repetition of stale and common-place details. Undeterred, then, by scruples which would urge the impropriety of describing a disease so often and happily described before, I shall furnish a concise enumeration of its most remarkable symptoms, as they fell under my observation at Curacao. The impression made on my mind by the first case to which I was called, can never be erased ; the peculiar physiognomy, the insidious progress, the acute mental and bodily suffering, and the rapid and fatal issue, furnished a bedside lesson of disease, which not the ablest and most pointed descriptions can ever convey. The occurrence of this disease too in an island so dry and free from many of the sources of the disease, on which too much reliance has been placed, will not be deemed unworthy of record.

The yellow fever is endemial to this island at all times and seasons, but it occurs more especially in the calm and sultry months of the year. New comers from northern latitudes are its appropriate subjects; the natives and seasoned inhabitants being exempt from its attacks. When a great number of the first description of persons arrive at the same time, as sailors on board of vessels of war, or troops in transports, the disease generally prevails to some extent among them, and is then considere depidemical. Solitary individuals, arriving from time to time form the sporadic cases, which are more or less occurring during every month of the year. I had an opportunity in 1818, of seeing the disease in its epidemic form on board of two Dutch vessels of war.* Sporadic cases never failed to present themselves, at all times, during my stay in the island.

The disease generally invades the patient with the ordinary symptoms of fever: slight rigors, never amounting to a chill; pain in the head and loins; uneasy sensation about the stomach, and a general sense of weariness. Sometimes these symptoms are altogether wanting, and pyrexia seizes the patient suddenly: at other times they continue for several hours, and then are succeeded by increased arterial action, quick, full, and chorded pulse; great, and even pungent heat of skin; increase of the pain of the head, more particularly across the eye-balls; soreness and pain in the loins, stretching forward to the umbilicus, or shooting down to the thighs and settling in the calves of the leg, in which the patient likewise complains frequently of a sense of weakness. The countenance has a sad and characteristic appearance: the eyes are inflamed and watery; the tongue is slightly covered with a whitish mucus, and is most frequently moist. The skin is sometimes of a dingy hue, mostly not altered in its appearance, and frequently it is covered with a slight moisture. The stomach is very uneasy, experiencing a sense of vacuity, sometimes of hunger, and is generally painful on pressure from without; in some cases it retches even at this early period, and if vomiting be superadded, the prognostic is fearfully certain. The bowels are costive, and respond with difficulty to the action of purgatives. The urine is scanty. and what is voided is reddish and clear. The mind in general is serene, but fearful of the event.

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* I was at that time a student of medicine, and was indebted for much and highly valuable information to my respectable friend Dr. Cabrera, a citizen of Venezuela, alike distinguished for his professional skill, and for the eminent devotion he bears to the rising liberties of his country.

These symptoms continue increasing for forty-eight or sixty hours, when relief seems approaching in a temporary alleviation of the more violent symptoms. The febrile heat and pain yield and frequently go off altogether. But when the stomach has once been disordered, there is no cessation of its sufferings. This second stage has not inaptly been termed, by Moseley, *Metaptosis;* it is the token that re-action is exhausted, and the system is undergoing a rapid change, from a state of excitement to one of collapse and passive suffering. The third or last stage accordingly developes itself sooner or later in the sunk and irritable pulse, though in some cases it is little changed; the low temperature of the body; the approaching coldness of the extremities, and yellowish suffusion appearing first about the eyes and angles of the mouth, and on blistered surfaces, particularly in moments when the patient, either from vomiting or any other exertion is exhausted, then diffusing itself over the neck and breast and whole body. The patient complains now of increasing uneasiness about his stomach, and is urgent for cold drink, which is no sooner taken than it is rejected, and accompanied with increasing quantities of black membranaceous flocculi swimming in the fluid, and of a slate-coloured sediment at the bottom. He is frequently free from all disturbance, and inclines to doze, but he no sooner falls into a slumber, than the perturbation of his stomach becomes perceptible, and vomiting is brought on. He now lies on his back, with his knees drawn up in the bed, and is intolerant of any covering about his head: he occasionally sighs, and tosses his hands to and fro; is anxious for something hard upon which to press, and frequently leaves his bed with insane violence, and grasps the floor or the bed, and seems to experience relief from the indulgence. Hiccough, increase of black vomiting, dark and fetid stools, hemorrhage, and perhaps a convulsive fit end his acute sufferings.

Such is the general outline of the train of phenomena characterizing this disease, whose mortal progress it is the severe duty of the humane physician to watch with scarce a hope of any other reward to his labours and anxiety except an addition to his melancholy experience.

All cases are not, however, thus distinctly marked. Patients sometimes labour for a day or two under no other symptoms than those attending an ordinary cold, for which a dose of salts from the medicine chest is prescribed; their eyes are watery, they feel disinclined to move about, and lie down listless and unconcerned of what is passing about them, when, of a sudden, retching and vomiting come on, and perhaps before the physician has time to appear they are in the agonies of death.

On the treatment I have nothing new to offer, and shall not say any thing. Emetics were, however, proscribed, and blisters were by some thought hurtful. In two cases, which threatened to put on the most violent form of the disease, the most active depletion by the lancet, and brisk cathartics led me to flatter myself that I succeeded in arresting the disease.

1. As to the character of the fever, it is evidently an ardent continued fever that vents itself on the stomach and brain, and originates only in places near to the sea. It is distinct from the bilious remitting fever that arises in hot weather, in warm climates, and in every situation that is exposed to the action of heat and moisture. These diseases have been confounded, it is true, but they are characterized by so many differing circumstances, that it appears very strange how they could have been ever identified. In the yellow fever the liver is not the cause of the disease, nor is it primarily affected. as is the case in bilious fevers;* there is no remission of the fever as in

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* In addition to what I have said in a preceding page on this subject, I may here add that a further confirmation is derived of the liver not

the bilious remittent, the abatement of the excitement which takes place in the metaptosis being no other than, as it has been accurately described by Lining, a total cessation of the fever. To the yellow fever, the newly arrived northern man is alone subject, whilst the native and seasoned inhabitants are alike, with the former, obnoxious to the bilious fever. The yellow fever attacks the same person once only. excepting in cases where a temporary residence in a cold country has, in the interim, predisposed the constitution anew to an attack of the disease,* whereas the West Indians are liable to repeated, and even annual attacks of their bilious fevers. The treatment too, differs in the two diseases: emetics, which are highly proper in the bilious remittent fever, being most generally fatal in yellow fever. On each of these highly important heads, abundance of testimony could be adduced, but the limits to which I have prescribed myself will barely permit me to refer to the pages of Moseley, Jackson, Lining, and Mc Arthur, from whose ample experience the above results and conclusions have been drawn. But the locale of the two diseases differs, and this leads me to notice,

2. The cause of yellow fever. I am aware that I am treading on dangerous ground, when I approach this subject. The immediate cause of yellow fever is far from being known; but does not this very circumstance distinguish it sufficiently from the bilious remittent, whose source is indubitably heat and marsh miasma? That heat is no less necessary to the production of yellow fever, cannot be denied; but it is by no means apparent that miasma from marshes is a cause of the fever. For if it were, the yellow fever would be co-extensive with the bilious remittent, which is by no means the case. The latter pre-

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being necessarily affected in yellow fever, from the circumstance that in dissections of this disease in the United States the liver is seldom or never found diseased.—See *Dissections*, by Drs. Physic and Cathrall of Philadelphia in 1793.

* It is the predisposition thus newly acquired by the inhabitants of northern latitudes, during their winter, that exposes them to repeated attacks of yellow fever.

vails under the influence of heat and moisture in the interior as well on the sea-coast of those countries in which it is endemic. But the yellow fever is confined to the neighbourhood of the sea; in the inland districts of the larger islands, and of the American continents, it is unknown. This difficulty it has been attempted to get over by the assertion, that they are various grades of the same disease. But from the locale assigned to each, it must be evident that the cause is entirely different, and hence the logical inference is just, that the effects are likewise different. Besides, there is a sufficient number of facts on record to prove that this fever occurs in hot latitudes at sea, before new-comers have approached the land;* and hence it has been considered in these cases a disease of pure excitement from heat alone. The occurrence of the disease in perfectly dry situations, as at Curacao, tends to the same point; nor does the prevalence of the disease as an epidemic, detract from the force of this conclusion, as it implies a difference in the number only of those simultaneously exposed and attacked, not in the nature of the cause or origin of their disease. The knowledge which we possess on this subject at this day resolves itself into the following data: that yellow fever prevails in tropical and tropicoid climates, and among the agents necessary to its production, the presence of heat and the neighbourhood of a sea atmosphere are alone well defined and understood.

3. If I approached the cause of yellow fever with fear, I must speak with delicacy of its *contagious* property. In the cases which I have seen of this disease, I never could perceive the existence of this quality: sporadic cases were certainly free from it under all circumstances, and the epidemical cases which have been suspected to possess this virus, were only observed to impart it within the sphere of the original source of the disease. But there are so many contradictory facts recorded on this head, and my experience with regard to it is so limited, that it would be daring indeed on my part to offer any

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* Lind. M'Arthur, &c.

conclusion. Happy shall be his destiny, to whom it is reserved to analyze and trace the causes which have involved this subject in so much mystery and discussion; and more happy he, who can reconcile the jarring opinions and discrepant views of such men as Bancroft, Chisholm, Rush, Miller, and Hosack.

ART. III. *A case of* COMPOUND PREGNANCY, *accompanied with prolapsus Uteri.* *By* MARTIN L. BRYAN, M. D. Fairfield, Herkimer County, State of New-York.

MRS. C. N. previous to the age of twenty, had been accustomed to labour in the field, and was uncommonly robust and hardy. About that time her health began to decline; menstruation was performed with less regularity; the quantity of the secretion was diminished, and a leucorrhœal discharge was observed, particularly about the menstrual period. At length, as she was carrying a heavy burden, she "felt something give way," and was immediately seized with excruciating pains in the back and hips, an uneasy sensation in the groins, and difficulty of voiding urine, with cough and pain in the side, accompanied by fever. The parts of generation, nates, and thighs, soon became inflamed and very tumid. The fever continued for several weeks, and the inflammation and swelling for months. The attending physician bled her frequently, and used cooling applications; but, as she concealed from him the seat of her greatest distress, no examination was proposed. After about a year, she recovered so as to be able to walk, by inclining her body a little forward, though she was still liable to a return of the inflammation and swelling, from any considerable exertion.

About three years after this attack she was married. The expectations of her husband seemed disappointed, and his enjoyment incomplete; and the same misfortune which was to him a cause of discontent and dissatisfaction, proved to her a source of pain and disease. In this situation of things, in March, 1818, about three months after the marriage, I was consulted in her case. At that time I received from her the foregoing account. She expressed the greatest anxiety for relief, and preferred death to a life so miserable. I immediately proposed an examination, to which she readily consented. I found the uterus apparently enlarged, and filling almost the whole cavity of the pelvis. The os uteri was resting on the perinæum just behind the fossa navicularis. The vagina was folded, thickened, and almost as hard as the walls of the uterus. I could not introduce my finger, in any direction, more than one inch and a half. The parts were sensible to the touch, and she complained of pain during the examination. I endeavoured to elevate the uterus, but found it completely fixed in its situation. Having directed her to keep her bowels loose, and apply cold water freely to the parts, I left her in a state bordering on despair.

She continued subject to frequent attacks of inflammation and excoriation of the vulva and surrounding parts. In February, 1820, during a suppression of the menses from cold, she had an attack so severe as seriously to threaten her life. From this, however, she partially recovered, and in April following, the menses returned. About this time she became pregnant, and her health improved. Although the symptoms of pregnancy were more troublesome in her case than I have usually found them, yet for three months she continued to be more comfortable than she had been for some time previous to impregnation. I suggested her probable situation; but she refused to listen to the suggestion, supposing it impossible for a woman in her situation ever to conceive. At the commencement of the fourth month, her afflictions seemed to return with increased severity. In the fifth month the symptoms became alarming. Fever, swelling of the labia, thighs, and hips, soreness in the epigastric region, and pain in the back, accompanied with a train of nervous symptoms, seemed to indicate that the necessary rising of the uterus was breaking up the connexions, which had retained that viscus in its unnatural situation.

From this time she was unable to walk, till about the end of the seventh month.

On the 14th of December, 1820, I was called at five o'clock, A. M. to attend her in parturition. She had passed several very restless nights, her abdomen was unusually distended, and she felt a constant pain in her back and bowels. There seemed to be a tonic action of the uterus, though there was no appearance of regular labour pains. I let blood, directed her bowels to be freed by an enema, and left her. I returned about three o'clock, P. M. and found her very restless. The pains had become almost insupportable, but were still irregular. On examination, I found the os uteri considerably dilated, and the feet of a child presenting, with the toes towards the right side. I waited at least an hour, in expectation that the pains would become more regular, and the feet descend without assistance. The os uteri was dilated, but the uterus acted feebly on the child. With the left hand I brought down the feet, and, as the thighs were passing, by gently turning them, I brought the face in the direction of the right sacro-iliac junction, and she was soon delivered of a male child.

As there was sufficient evidence that another child was contained in the uterus, two ligatures were passed around the cord. The child being removed, I introduced my finger into the vagina, and found another set of membranes, through which a child's head could be distinctly felt, in the second presentation of Baudelocque. Though the pains were feeble, she was delivered of a daughter, in about two hours. The abdomen still seemed to be considerably enlarged, and, on examination, I found the membranes of a third child, which presented its left shoulder to the os uteri, its head resting in the right iliac fossa, and its back being towards the anterior part of the uterus. At this time my patient seemed almost exhausted. After administering cordials, I cautiously introduced my left hand into the uterus, and succeeded in carrying up the shoulders and bringing down the feet. The uterus now seemed incapable of making any effort to expel the child. I directed an assistant to keep up friction with spirits over the region of the uterus, and to make pressure on its fundus when there were appearances of pain. By these means, and the cautious use of extracting force, she was soon delivered of the third, a male child. The umbilical cord being tied as before, I passed my finger into the vagina, and was astonished to find another set of membranes, through which I felt the knees of a child, with the thighs towards the mother's right side. As my patient seemed sinking, I directed volatiles to be applied to the nose, and gave her a little wine and water. To wait for nature to accomplish the delivery, would evidently be attended with danger. I introduced my hand and brought down the feet. I waited a short time. There was scarcely the slightest appearance of pain. Friction with spirits, and pressure on the uterine region were continued. I used gentle extracting force. When the head had descended into the pelvis, I carried up my finger and hooked it into the child's mouth; but all the force which I thought it prudent to use, was not sufficient to accomplish the delivery, and the mother was utterly unable to assist. Anxious for the safety of the child, I introduced one blade of the forceps, by means of which I was enabled to complete the delivery of the fourth child, within six hours from the birth of the first.

The situation of both mother and child was now very alarming. Whilst I was using the common means for the resuscitation of the child, I directed the assistants to keep a constant pressure on the uterus, to apply a cloth, wet with spirits and water to the vulva, to administer cordials, and apply volatiles to the nose. As soon as the child began to gasp for breath, I gave it to an assistant, to be immersed in a warm bath, while I directed my attention to the mother. I found blood flowing from the uterus in considerable quantities. The placentæ were detached from the uterus, and being connected together, were removed without difficulty.

Notwithstanding every means was used to induce uterine contraction, the removal of the placentæ was followed by a gush of blood. Recollecting the practice of Professor Willoughby in such cases, I wet my hand with spirits, carried it into the uterus, and gently tapped its walls with my fingers, and although life could scarcely be detected, yet the uterus contracted, and the hemorrhage ceased. A broad bandage was immediately applied about the abdomen, a little wine and water with laudanum administered, and heat applied to the extremities. In about two hours the pulse at the wrist began to rise. I directed the attendants to keep her quiet, with a cloth wet in cold spirits applied to the vulva, and ordering her a little wine and water occasionally, with powders of acetate of lead and opium, to be given if the hemorrhage should return, I left her.

The history of the children is briefly this. They were born on the evening of the 14th of December. Though they were extremely emaciated, they appeared to be doing well until the evening of the 15th, when they all died within the space of two hours. I attributed their loss to the inclemency of the season, and the want of proper provision for their comfort. They were wretchedly clad, and had nothing but a miserable hovel to protect them from one of the severest storms of winter.

The weight and dimensions of the children were as follows:

	Sex.	Weight. lb.	oz.	Length inches.	From nape of the neck to crown.	From one pariet. protub to the other
First,	a boy	3	8	17½	3¼	3⅛
Second,	a girl	2	4	14¾	3	2¾
Third,	a boy	3	4	16½	3⅛	3
Fourth,	a girl	2	14	15¼	2⅞	2⅝
Aggregate		11	14			

The mother was attacked with puerperal fever, from which she slowly recovered. She was unable to walk without assistance, until the middle of May following. The menses returned in April, but did not appear a second time until the 12th of June, after an interval of six weeks. About this time she became again pregnant, and, on the 16th of March, 1822, became the mother of two fine healthy girls. On the ninth day she suffered considerably from uterine hemorrhage, and still remains in a feeble state of health.

Each child had its separate involucrum, the placentæ being united by their membraneous edges forming in this way one united mass, weighing, when washed, two pounds ten ounces. It was deposited in the museum of the Medical College at Fairfield, where it has been preserved for the use of the professor of Obstetrics.

ART. IV. REPORT *on the* YELLOW FEVER *which prevailed in* NEW-YORK *in* 1822. *In a letter to his honour the Mayor of New-York. By* JOSEPH BAYLEY, M.D. Health Officer of the Port of New-York.

New-York, December 9, 1822.

MESSRS. EDITORS,

I enclose, for insertion in your Journal, a Report on the Yellow Fever which prevailed in this city during the last season. This report will shortly be laid before his honour the Mayor.

Yours, respectfully,

JOSEPH BAYLEY.

TO THE HONOURABLE STEPHEN ALLEN,

PRESIDENT OF THE BOARD OF HEALTH.

MY time has been so fully employed with quarantine duties, and recently with the removal of my family to the city, that I have not had leisure until now to examine the necessary documents, which would enable me to state some facts, and offer some observations on the important subject contained in your letters, and I am persuaded that you will accept of these excuses for my seeming neglect.

In your high and responsible station of Chief Magistrate of this city, and of President of the Board of Health, the cause or source of the deplorable calamity with which we have been afflicted, has deservedly claimed your unwearied attention and close investigation.

The first vessel mentioned in your letters as one of the probable sources of the late pestilence, is the ship Shamrock. She arrived at the quarantine on the 11th of June from Havana, via Matanzas, and after the cargo had been discharged, and the vessel purified, she was hauled to the wharf at or near Rector-street, on the 24th of that month. It is my opinion, that this vessel was not the cause of the disease, for the following reasons:

First. Her crew was composed of eleven *eastern seamen*, who are more liable to the diseases of warmer climates than southern sailors, yet they enjoyed uninterrupted health, from the time they sailed from Wiscasset last February, until their arrival in this port, and I have not heard that any of them were sick while they remained here.

Secondly. When this vessel was at Havana in April and the beginning of May, that place was declared to be unusually healthy, the truth of which was confirmed by the fact, that of one hundred and ninety-two persons, the crews and passengers of thirteen vessels from Havana, who arrived here in the month of May, only one person was sick during the voyage, and his disease was diarrhœa.

Thirdly. The Shamrock sailed in ballast from Havana, May 9th, for Matanzas, which place was also reported to be healthy; there she was loaded, and proceeded, June 1st, from thence to this port. An evidence of the healthiness of Matanzas, while the Shamrock was there, appears from the circumstance of one person only being sick out of one hundred and twenty-three, who came here in ten vessels from that port in the months of May and June, and he died of consumption. I was also informed by one of our most respectable merchants, who had resided several months at that place, and arrived here a few days after the Shamrock, that Matanzas had been perfectly healthy for many months, and was so when he left there. These facts ought to convince us that this ship was not infected.

The schr. Florida is considered by many persons as having been the source whence the deadly malady arose; not that she was a sickly vessel on her late voyage, but because she is supposed to have introduced the yellow fever into St. Augustine the year before, after her return from Havana, at which place, or on her passage from thence to St. Augustine, several of the crew were sick with that disease. Capt. Johnson, her present commander and owner, informed me, on his arrival here a year ago from St. Augustine, that the first persons taken sick with yellow fever at that place, were those who washed the clothing which he used during his illness, and who lived in the house where the clothing was sent. Now admitting the foregoing circumstances to be true, still we must allow, that the frosts which occurred here from the eighth to the twenty-sixth of November, while she was in this port, must have eradicated the remains of any infection which might lurk in this small and shallow vessel. She is less than six feet deep in her hold, and only thirty-three tons burthen. She sailed from New-York November 26, 1821, and arrived here June 21, 1822. While she was away, she was employed exclusively in aiding vessels wrecked on Florida reef, from which place she made several trips to St. Augustine and St. Mary's. She was also once or twice off the harbour of Havana for a day or two last February. Capt. Johnson's only business there, was to receive payment for transporting troops or emigrants from Florida to Havana the preceding year: he did not go into the harbour, as it would subject him to pay port-charges for his vessel. From the time that she sailed from New-York until her return, a period of seven months, not a person was taken sick during her absence, although the crew were changed more than once. This, certainly, is a strong proof that no infection remained in her, or it probably would have shown its effects by occasioning sickness among her crews, as they had been on board several months, while she was in a southern climate. Her cargo, which merely put her in ballast, consisted of a little old copper and iron, a small quantity of snuff, junk, and fruit. Her hatches were off all the passage. Her bottom was so worm eaten. that she required constant pumping. After the Florida was purified she was hauled to the wharf near Rector-street, on the twenty-fifth of June.

The schooner Nile has also been mentioned as a suspicious vessel, but as she did not arrive at the wharf near Rector-street until ten days after the fever commenced, it is unnecessary to examine any further into her situation, than to remark, that she was so perfectly clean, that the most scrupulous could not find fault with her. None of the crew were sick during the voyage, and the port she came from was healthy while she remained there.

These are the only vessels supposed to have been infected, which were at the wharves near Rector-street, that I have heard of. If there are any others which have been at that place, whose condition you are desirous of having examined into, I will with pleasure give you all the information in my power.

The local nuisances at or near Rector-street, to which pestilence has been attributed, are very limited in extent, when compared with very many parts of our city, which have been totally exempt from the fatal fever of the present season. Its northwestern exposure renders it some degrees cooler, than it is on the southeastern side of the town. The made ground there is composed of the best materials, and that which was dug up to the depth of three feet a month ago at the foot of Rector-street, to connect the new pier erected there to the bulkhead, was, on close inspection, found to be clean gravel and stone-cutter's chips, and entirely free from any offensive smell, although this is the very place, which many imagine was impregnated with the pestilential materials supposed to be conveyed in the rain water that falls on, and filters through Trinity church-yard, and which being there deposited, have been the cause of the late pestilence.

The peculiar nature of contagious or infectious air has not yet been discovered: its existence in any place is known only from its effects, and from these alone can we at present fairly draw our inferences.

The facts are numerous and indisputable to prove, that the yellow fever has been communicated to individuals who have visited vessels on board of which one or more persons have had that disease, or which vessels came from a place where it prevailed; and they have imbibed the infection, not only while the cargoes were on board, but after they were discharged, and the vessels thoroughly cleansed. A noted instance of this fact occurred this year on board of the United States brig Enterprize. After the sick had been removed to the hospital, the well were landed, that they might not be any longer exposed to the infected air of that vessel, and to afford an opportunity of purifying her, which was done by slaking lime in her limbers, and white washing her throughout, not omitting her iron ballast; in which operation three casks of lime were used; and she was well ventilated by four windsails constantly in her hatchways. The crew were so intemperate during the five days they remained on shore, that we were under the necessity of sending them again on board. The brig had been purified in the interim. But it was soon evident that she was still an infected vessel, for in six days after the crew were sent on board four men were taken sick with yellow fever, and in the course of five days seven more had the same disease, making a fourth of all the men on board; and five of the eleven taken sick died. These persons must have been infected after their return to the brig, and subsequent to her purification, otherwise the disease would have been excited in them as it was in the case of some of their shipmates, from their irregular living, and exposure to the weather, by lying on the ground at night. Those men must have been more predisposed to receive infection after their return to the brig, than they were previously to their landing: in consequence of having lived in a pure atmosphere, and having been debilitated by repeated intemperance.

This fatal evidence of the cause of the disease still lurking in the Enterprize, induced us to have the crew brought on shore again, and recommence the purification, which was done by using two more casks of lime, and letting into her hold daily several feet of water, and keeping up windsails. Six men were left to take charge of her, who were directed not to sleep below deck, and whose duty was to pump out the water and trim the windsails. On the second of August, twenty-five days after her arrival, and after repeated white-washing, letting in water, and constant ventilation, one of the sailors obtained permission of a lieutenant to take his wife on board; this woman was taken sick with yellow fever on the ninth, and died in the Marine Hospital on the eighteenth of that month. In the case of this vessel we have a positive proof that this disease comes from abroad, and that its infection continues to act after an abundant use of lime, being ten times the quantity employed in white washing a house of the same dimensions, besides the immense quantity of water let in and pumped out, and a constant supply of fresh air conveyed into her hold by windsails. Here no animal or vegetable matters were to be seen, or otherwise detected, whose decomposition could keep up this poisonous air, especially after she was first cleansed.

It is asserted by many persons that animal and vegetable decomposition is the cause of the yellow fever in the West Indies; and as our climate is equally as warm in summer as it is in those islands, the same effects must follow like causes here as well as there, consequently the yellow fever may also be generated in our city. This reasoning is philosophical, and if the premises are well founded, we should not hesitate to submit to the conclusions drawn from them; but for the sake of argument, I would ask if there is no difference between the duration of heat there and here? Does not this circumstance make a very material difference in the productions of the two countries: and why may it not make as great a difference in the products evolved in their decomposition?

Wherein the constituent parts of an infected atmosphere differ from a pure one is unknown. Until this mystery is developed, and the gases which arise from the decomposition of animal and vegetable substances here, are proved to be precisely similar to those which are supposed to generate the yellow fever in the West Indies, we cannot, for the reasons assigned above, consider them to be the same.

This theory of the disease is not supported by the facts furnished even in our own city. If the reasons upon which it is founded are correct, the yellow fever should occur, at the same time, in all those places of our city where nuisances exist, in the interior, as well as along our wharves; and especially at all the points where it had before commenced, as the same materials which had once generated it, could not fail to reproduce it; and where receptacles of filth most abound, there the infection should be extended to a greater distance, and the virulence of the disease greatly increased. Seldom a year would pass without our being smitten with pestilence; and its annual return would be usually at or near the same time of the year, only a little varied by the previous state of the weather.

It is well known to our inhabitants, that as often as we have been visited by that awful calamity, it has never commenced at the same place, and never has it began in the interior of our city. It has commenced its ravages as early as June, and as late as September, in seasons nowise remarkable for their peculiarity; while we have some years escaped the pestilence, when the combined action of heat and moisture has been excessive; which causes are universally admitted to give increased activity to the decomposition of putrescent substances. In the present year we have witnessed its spreading over the highest, cleanest, and most airy part of our city, and far from the spot where it commenced.

As it does not appear that the vessels above referred to, or the local causes that existed in Rector-street, afford a satisfactory explanation of the origin of the late pestilence, I beg leave to call your attention to another foreign source, that has hitherto received too little attention, but which, on close investigation, will probably appear more adequate to its production than either of the foregoing. It has been so repeatedly proved that vessels from sickly ports have given rise to the yellow fever, where no local causes existed for its production, that even those who believe in its domestic origin admit that it may be imported. If, then, pestilential air escapes from a vessel at the wharf, while the cargo is discharging, may not the cargo of such vessel, immediately transported in lighters, under particular circumstances, retain a portion of pestilential air, and the cause of disease be conveyed to the city in this manner? The following statement of the transportation of cargoes from sickly Havana vessels, or which sailed from that port when yellow fever prevailed there, has been carefully compiled from the records kept by the revenue officer at quarantine.

June	14	3	lighter loads from the	brig Rapid,	363	boxes of sugar.
	15	2	do.	do.	189	do.
	28	1	do.	brig Spanish Soldier,	77	do.
	29	I	do.	do.	100	do.
July	1	2	do.	brigs Span. Soldier & Abeona	177	do.
	2	3	do.	brigs S. Soldier, Abeona, and Ambuscade,	260	do.
	3	3	do.	do. do. do.	181	do.
	5	4	do.	do. do. do.	216	do.
	6	5	do.	do. do. do.	384	do.
	8	4	do.	do. do. do. and ship Eliza Jane,	317	do.
	[illegible]	I	do.	do.	106	do.
Aug	5	1	do.	brig Packet,	70	do.
	6	I	do.	do.	70	do.
	7	2	do.	do.	145	do.
	8	1	do.	do.	75	do.
		34			2730	

From the above particulars it appears that 2730 boxes of sugar were transported in thirty-four lighters from several sickly Havana vessels, (and those not sickly were navigated by persons who had made frequent voyages to that port,) between the fourteenth of June and eighth of August, and landed at the wharves, within the limits of one hundred and twenty yards on each side of Rector-street: but the circumstance to which our attention ought chiefly to be directed, is the important fact, that more than two-thirds of the whole quantity, amounting to 1918 boxes, the entire cargoes of the three brigs

Spanish Soldier, Abeona, and Ambuscade, and part of the cargo of the ship Eliza Jane, were conveyed in twenty-four lighters, between the twenty eighth of June and ninth of July; and even nineteen of the twenty-four in the short period of six days, from the second to the eighth of August. The lighters were generally loaded before noon, at which time the weather was frequently calm, consequently the infected air adhering to those boxes could not have been completely driven off in passing them from the hold of the vessel to the hold of the lighter. The heat of the weather in the shade at Staten Island, between the twenty-eighth of June and ninth of July, was above eighty degrees at two P. M., and upwards of seventy-seven at eight A. M. and six P M.; and for the whole period between eight A. M. and six P. M. the average was more than seventy-eight degrees. It has been erroneously stated in some of the public prints that it was my opinion that the pestilence had been conveyed in boxes of sugar. I never entertained such an idea; but I conceived it possible that infected air, shut up in the hold of a vessel during a West India passage, would as readily pervade, and be retained in the spongy texture of rough pine boards, of which those boxes are made, as the more dense structure of smooth oak planks, of which vessels are built; and that several lighters, loaded with such cargoes, being landed daily at or near one place, would probably convey as much pestilential air as a single vessel discharging at the wharf, and which has been known to produce the disease. In the instance before us we have the cargoes of three vessels discharging at the same time, and near the same place.

It may be asked, if the cargoes from sickly vessels, or from sickly ports, have not been brought to other parts of our city where no evil has been produced from them; and if the pestilence can be conveyed in that manner, how could such places be exempt from it?* To answer this question, I have ascertained the amount of produce from Havana, which has been landed at all parts of the city, other than was discharged near Rector-street, and find by the revenue officer's reports, that 1518 boxes of sugar, and 361 hogsheads and 107 barrels of molasses and sugar, have been transported in twenty-seven

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* The cargoes of the following vessels from Havana were discharged at other parts of the city than at or near Rector-street:

At Old-Slip.

June 20	1	lighter load	from schooner Retrieve,	60 boxes of sugar,	50 bags of Pimento.
22	2	do.	from schoon. Retrieve,	172 do.	20 hhds. do. and 50 bags of Coffee.
28	1	do.	from schoon. Cerena,	30 do.	26 hhds. do.
29	1	do.	do.		35 do.
July 1	1	do.	do.		35 do.

At Fulton-street Wharf.

June 21 22 24 25	1	do.	from the brig Venus		256 hhds. Molasses.

At Stephen's Wharf.

June 27	2	do.	from the schr. Virginia Packet	135 boxes of sugar.	
28	2	do.	do. do.	191 do.	
29	1	do.	do. do.	82 do.	

Between Old and Coffee-House Slips.

July 12 13 16	1	do. each day,	sch. Kennebeck Trader,	200 do.	115 hhds. and barrels do.

Near Old Slip.

18	1	do.	brig Fame,	108 do.	
19	1	do.	do.	47 do.	
20	2	do.	do.	140 do.	
22	1	do.	do.	106 do.	
23	2	do.	do.	141 do.	
Aug. 1	1	do.	brig Packet,	84 do.	
5	1	do.	do.	67 do.	
	27			1565 boxes—	497 hhds.

The crews of those vessels, except the brig Venus, were healthy during the voyage: that brig lost one man, June 7. Dr. Morrel, a passenger on board, it is said, died of bilious, and not yellow fever.

lighters, between the twentieth of June and the eighth of August; (after which time no cargo was brought here from sickly vessels, except such as was purified by washing the casks, &c.)

But the greatest number of lighter loads, landed at or near one place in six successive days, did not exceed seven, which occurred only in a single instance. From the eighteenth to the twenty-third of July these lighters conveyed the cargo of the brig Fame, on board of which vessel no person had been sick during the voyage; whereas nineteen lighter loads, from three vessels, were discharged in the same number of days, near Rector-street, from the second to the eighth of July. This, in my opinion, is an obvious difference; for a small quantity of pestilential air may be inoperative by mixing with a large portion of atmospheric air, while a greater quantity, existing in a more concentrated state, proves fatal. The fever which took place at the quarantine ground in 1821, when many vessels were there driven on shore, is an example directly in point: it was evident that the foul air issuing from those vessels at the same time contaminated the pure atmosphere of the country, which occurrence never before happened from a small number at the wharf. Other circumstances, that might make a difference between the cargoes which were landed at Rector-street and other places, could have been ascertained, when the vessels arrived here, if I had been aware that any such particulars would have been of any practical use: such as the place where the vessel laid in the harbour, as some positions being healthier than others; and whether the cargo came immediately from the country, or had been stored in a sickly part of the city. It is a well authenticated fact, that vessels coming from Havana, loaded with molasses, are healthier than those loaded with dry sugar, although the bilge water of the former is much more offensive than that of the latter: the reason of this difference in the healthiness of their crews is attributed to the circumstance that the first are loaded at a village called the Regulars, situated on the opposite side of the bay to the city of Havana, where it is generally healthy.

My object in this investigation is not to support any theory on the subject, but to endeavour to come at the truth, and learn if any reason could be assigned that would account for the difference, which appears to have been produced by the cargoes landed at Rector-street and other places, that measures may be adopted to guard us in future, if the disease has been introduced in this manner. Although the law requires the health officer's permit before the cargo of a sickly vessel can be brought to the city, yet it has been the practice heretofore to detain at quarantine only those articles which were damaged or generally supposed to be susceptible of retaining infection.

The commerce of this port with sickly West India ones is so extensive, that all the cargoes brought here from them, cannot be stored at quarantine without providing additional buildings; and the merchants could not examine them there without being exposed to the foul air of damaged and infected articles, as well as to sickly vessels at the wharf; this would produce considerable embarrassment in their business, by impeding or preventing the sale of their property. It would therefore be proper to discriminate between articles which come from ports where the yellow fever prevails, as they are more or less liable to retain infection, that those least liable might be stored at an intermediate place, between the city and Staten Island, to be ventilated for twenty or thirty days before they are brought to the city, and where the merchants could have free access to them with little hazard. West India produce, coming coastwise, which has not been landed in the United States the same length of time, and which is not susceptible of purification, except by ventilation, ought also to be taken to the same place.

I conceived it to be my duty to offer to you my views on this particular point, although it may appear trivial to some; yet on this highly important and interesting subject, which is wrapt up in so much mystery, and in which the lives and property of our fellow-citizens are involved, every circumstance which in the smallest degree can throw any light upon it, should be unreservedly communicated, especially if it can be of any practical use.

During the late pestilence, eighty-five persons were sent to the Marine Hospital from the city and fort Richmond, forty of whom died. Sixty-five had yellow fever, and of this number thirty-seven died, eighteen of whom had black vomit; from which it appears that the recoveries were in the same ratio as in the city. This fact ought to remove the erroneous opinion entertained by the public, that the sick sustained much injury in being sent to Staten Island. It is readily admitted, that sick persons, surrounded by their relatives and friends, and supplied with every comfort, might be injured by their removal to any public institution, particularly to a Lazaretto, in consequence of the shock given to their feelings. Such, however, was not the happy condition of the persons sent to the Marine Hospital. They were generally destitute, without attendants; and living in the infected district, their removal to a healthy place, where they could obtain what their situation so much required, must have contributed to their comfort, and in many instances to their recovery.

The characteristic symptoms of the disease in those persons who were sent from the upper and lower infected districts, and those from the United States' brig Enterprise, were the same, with similar variations in acuteness, as are observable in other febrile diseases. The first or inflammatory stage in those from the Enterprise was generally more violent and longer protracted, than in those who were sent from the city; which probably was occasioned by the former being more robust and plethoric than the latter, nearly half of whom were females, but we had not a frequent opportunity to judge how violent their fever was at its commencement, as most of them had passed the first stage before they were removed.

We did not observe that the malignancy of the disease was in any degree lessened in those persons who contracted it at a distance from the place where it began, nor that its violence was in any measure mitigated in those who were infected a short time before the pestilence terminated.

The more I see of yellow fever, the stronger is my conviction, that it is a different disease from bilious remittent fever. As they are both diseases which prevail in warm climates, and often exist in the same place, it is supposed that they have a common origin, and this no doubt is the reason why the great majority of physicians view them as the same disease. This was formerly my opinion, but which has been long changed, by observing the symptoms and termination of both diseases at the Lazaretto. The one is brought from the West Indies, the other is usually contracted in our southern ports, where extensive marsh exhalations abound, and it is unquestionably produced from that cause: this opinion is confirmed by its frequently terminating in an intermittent, which is the common disease of low and marshy countries. But of the fifty recoveries from yellow fever this year at the Lazaretto, not one of them had intermittent fever. To ascertain the relative proportion of deaths to recoveries between those two diseases at the Marine Hospital, I have examined the register of that institution since it was opened at Staten Island in the year 1799, to the present period, and find recorded in it 508 cases of yellow fever, 283 of which terminated fatally, making the deaths as 4 to 7, and 308 cases of bilious remittent fever, of which 86 died, making the deaths only 2 in 7. This very great disproportion in the fatal termination of the two diseases is in itself a very obvious distinction; moreover I have witnessed the greater part of those 86 deaths, none of whom had the coffee-ground black vomit.

Many persons who believe in the local origin of the yellow fever in our city, attribute it to the gas arising from the decomposition of animal and vegetable matters generated at some depth below the surface of the earth. It was readily perceived by those persons, that if they ascribed it to the putrefactive process, which takes place on the surface, then its effects would be felt much earlier in the season, than at the last of August or first of September, at which period the pestilence has often begun; for the heat of the weather was as great and often greater during the two preceding months, than it was when the disease commenced; consequently the noxious gases produced from such decomposition, must have been diffused in the atmosphere a much longer period, before its evil consequences are experienced, than facts will warrant us in allowing. But to account for the continuance of the disease in autumn from this cause, was a difficulty of still greater magnitude, as the evolution of gases from the decomposition of putrescent substances on the surface would be checked or prevented by cool weather.

Since then the gases from the decomposition of animal and vegetable substances on the surface of the ground, do not satisfactorily explain the origin and continuance of yellow fever, we were induced to examine into the more plausible theory, that the noxious gases which are said to give rise to it, were produced at some depth below the surface of the ground, which would better explain why the disease did not appear until the sun had been long acting on the earth and imparted its heat to it, and why it continued after cool nights had checked decomposition on the surface. For this purpose we made a number of experiments between the 18th of September and 26th of October, by burying the thermometer in the earth from three inches to seven feet, and letting it remain there from six to 12 hours. In thirty experiments made on different days at or near sunrise, the thermometer lying on the ground, stood at some point between 35 and 72 degrees, and when taken out of the ground after being buried twelve hours at various depths, from three inches to seven feet, it ranged between 44 and 67 degrees. To the depth of nine inches, the temperature of the earth was altered by alternate changes of atmospheric heat, but very little variation was observed in the thermometer when buried between one foot and seven feet, although the change was considerable on the surface of the ground. For instance, when the thermometer, lying on the ground, was at 52 degrees on the 24th, and 64 degrees on the 27th of September, it was 64 degrees one foot below the surface on both those days. Also, on the 5th, 9th, and 11th of October, when the instrument was laid on the ground, it was 51, 54, and 69 degrees, but when it was dug up from the depth of four feet the same days and at the same time, it was 64 degrees each day.

In twenty-two experiments made in the same manner, between one and three o'clock, P. M. the thermometer laid on the ground in the sun ranged from 76 to 91 degrees, but when buried for six hours in the earth at various depths, as between three and 12 inches, it varied from 56 to 76 degrees, according as it was placed near to or at a distance from the surface; for instance, on the 18th, 21st, and 23d of September, the thermometer laid on the surface of the ground, in the sun, was 84. 90, and 90 degrees at one o'clock, P. M. but after being buried six hours, and the sun acting all the time on the same spot, when the instruments were taken up at one o'clock on the same days, from the depth of three inches, stood at 74, and at the depth of six inches at 70. But when placed below the depth of one foot, it was as little affected by the heat of the sun as when buried in the shade, for it stood at 68 and 64 degrees, at various depths between one foot and seven feet, while the instrument laid on the same ground in the sun varied between 80 and 91 degrees. From those experiments made in ground not materially different from that which I saw dug up at the foot of Rector-street, it appears that the earth a foot below the surface, in the latter part of September and in October, varies very little in its temperature, when the sun-beams have been acting upon it for seven hours, or when it is shaded by twelve hours night, the heat being about 64 degrees.* This is a much lower temperature than I have ever heard would give origin to the cause of yellow fever; the heat of the earth one foot below the surface not being more than 64 degreee in September and October, we must therefore infer, that the continuance of the disease in autumn by the extraction of gases from the depth of several feet in the earth, is not a more satisfactory explanation of the cause of it, than that of the noxious gases emitted from the surface of the ground. We are not prepared to adopt this theory of the origin of yellow fever, unless adequate fermenting masses, producing internal heat and sending off pestiferous gases, are proved to exist constantly in the neighbourhood where pestilence prevails.

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* On the 24th and 25th of September two thermometers, which had been buried for twelve hours, to the depth of nine, twelve, fifteen, and eighteen inches, when taken up at sunrise, stood at 63 and 64 degrees, although the instrument lying on the surface was 52 and 50 degrees. But when the thermometers were buried at similar depths, for seven hours on the same days, the sun acting on the same ground all the time, they stood at 63 and 64 degrees at 2 P. M. although the instrument laid in the sun on the same place was 80 and 85 degrees.

I have thus presented to you a plain statement of facts, upon which I have formed my opinion of the introduction of the cause of the late pestilence, and offered a few observations connected with the subject, which I have been induced to do from a persuasion that they may be of some practical utility, and contribute to the improvement of our quarantine system.

With great respect,
Your most obedient
and very humble servant,
JOSEPH BAYLEY.

ART. V. CASE *of* MENTAL IMBECILITY, *arising from a contusion of the Anterior Tibial Nerve. By* JAMES ANDERSON, M. D. of New-York.

THE case which I now communicate for publication in the New-York Medical and Physical Journal, occurred in the practice of the late Dr. Richard S. Kissam, while I was one of his pupils, in the year 1818. As the eldest student, I was frequently requested by the doctor to give attendance on his surgical patients, and the case, the particulars of which I now send, was observed by me throughout its whole progress. I am inclined to think it sufficiently interesting, because it furnishes some striking facts concerning the connexion between the nervous system and the intellectual powers.

George ———, the son of G. T. ———, aged about fourteen years, a native of this city, of a plethoric habit of body, and of the nervous temperament, received an injury on the top of the foot, from a stone thrown with violence by one of his playmates. From the structure of the part upon which the momentum of the stone was expended, it contused that portion of the anterior tibial nerve, that lies between the metatarsal bone of the great and adjoining toe. The first effects of this injury were trivial, causing only a slight degree of pain and uneasiness upon motion, and were partially allayed, for six or seven weeks, by the ordinary domestic applications of tepid vinegar and water, liniments, &c. About eight or ten weeks after the accident, the pain increased, accompanied with a slight swelling of the part, and also with an extension of pain towards the ankle and leg. At this time Dr Kissam was sent for, who concluded that the existing pain was induced by an inflammation of the soft parts where the blow was inflicted, and unconnected with the nerve itself. His prescriptions were, accordingly, such as were indicated to lessen the sufferings of the foot: such as quietness, saturnine applications, and fomentations. From this treatment no benefit was manifested; the disease continued to increase in violence, and now extended itself further up the trunk of the nerve, and affected the adjacent muscles with spasms, which at times were mitigated in violence, but generally continued with about the same force. The muscles especially affected in this way were those directly supplied by branches from the anterior tibial nerve. The constitution was involved in this irritation; the pulse increased in frequency and force, though the digestive organs performed their duty with wonted regularity, if not increased strength. The pain increasing in violence and extent, blisters were now made use of, and constantly renewed. Saline cathartics and other purgatives were occasionally administered, and large anodynes prescribed with a view to obtain rest.

Having fairly tested the effects of the above remedies, and finding them ineffectual, closer examination evinced that the nerve I have mentioned was primarily the seat of irritation. It was now determined in consultation to have recourse to the most powerful antispasmodics and nervines. The pain had extended above the knee, and was almost insupportable. Assafœtida, cicuta, and belladona, produced no sensible relief; the symptoms became more formidable; the spasmodic action of the muscles was most conspicuous in the extensor longus pollicis pedis, and drew the great toe perpendicularly to the surface of the foot; the least effort to return it to its natural situation, or even the slightest touch would excite the most anxious sufferings: these lasted from three to five minutes.

At about three months from the time of the accident the whole system became involved in these spasmodic irritations, and his nervous energy enfeebled. The common sensorium was disturbed: he lost his reasoning and recollection; was unable to distinguish occasional visiters, or recognise even his parents, or any of the members of the family; his mind became imbecile and idiotic; he was deprived of the ability to read, or distinguish the letters of the alphabet. As the pain extended up and beyond the thigh, it spasmodically affected the action of the muscles of respiration; and at the invasion of each paroxysm of suffering his breathing became more frequent and laboured. This frequency of respiration was generally the first indication of approaching exacerbation. Though his distress was most acute, yet he gave no utterance to his feelings. While the paroxysms were on him he would roll his fist, and imitate the actions of a pugilist, but with much greater violence and rapidity, often striking his nearest and best friends, and all around him. If no person or thing was within the reach of his arms, the force of his actions would be lost in the air.

From the evidence of the local derangement of the tibial nerve thus involving the whole system, and the inefficacious results of all medicinal remedies, it was determined to have recourse to other means, and a surgical operation was agreed upon. A division of the anterior tibial nerve was deemed adviseable. This received the approbation of his parents.

Accordingly, the anterior tibial nerve was divided above the ankle, from a belief that it would more effectually cut off the connexion between the injured part and the nervous systers at large. The operation was performed in the presence of Dr. William Barrow, the consulting physician and surgeon, and myself. An incision was made about two inches in length just on the outside of the tibia, beginning about four inches above a transverse line drawn from one maleolar process to the other: the second incision was a corresponding division of the facia of the leg: the third separated the extensor longus pollicis pedis from the tibialis anticus, down to the anterior tibial vessels and nerves. The blood vessels being detached from the nerve, the nerve was divided by a bistoury, and about an inch of it cut out. Immediately upon this separation of the nerve, the extensor muscle of the great toe became so relaxed as to admit the toe to resume its wonted situation by the side of its fellow. There was an almost immediate alleviation of all those extraordinary morbid phenomena, that had exhibited themselves in the unfortunate sufferer. The local affections became less distressing, and in a short time wholly disappeared. The parts could be handled with impunity, and no excitement arise thence. There was a partial numbness of the top of the foot after the division, but the natural feeling was shortly after restored. The wound was dressed in the ordinary manner, by adhesive straps, and healed in the course of ten or twelve days. He has since enjoyed good health.

New-York, December 1, 1822.

ART. VI. AN *Examination of the Medico-legal Question, whether, in cases of* INFANTICIDE, *the* FLOATING OF THE LUNGS IN WATER *can be depended on as a certain test of the child's having been born alive.* By JOHN B. BECK, M. D.

IN the whole range of forensic medicine, there is not a question more important, and at the same time more difficult, than the one which it is proposed to discuss. It has divided the opinions of medical jurists from the earliest periods, and even at the present day it still remains the subject of controversy.

When it is recollected, how great and just an importance has been attached to it in trials for child-murder, and how embarrassing to courts and to juries have been the contradictory sentiments advanced concerning it by medical witnesses, the propriety of the present investigation cannot be questioned.

For the purpose of rendering the subject as distinct as possible, I shall first state the test, and then consider the different objections which have been brought against its accuracy.

If the lungs of a child which has never breathed be put into water, it is found that they are specifically heavier than the water, and of course sink. On the contrary, if respiration has once taken place, the lungs being specifically lighter than water, will then float. From these facts the general conclusion necessarily follows, that when the lungs of a child float in water, it must have respired, and therefore must have been born alive. And on the other hand, when they are found to sink, it is an evidence that the child has not breathed, and therefore was not born alive.

By the French this is denominated the *hydrostatic test* of the lungs, a name both convenient and appropriate, and I shall therefore adopt it in the following discussion.

Let us now see whether it is safe to trust to the evidence furnished by this test, by considering the different objections which have been urged against it.

I. It has been objected, that a child may have been born dead, and yet the lungs will float in water, from having undergone putrefaction, and therefore, it is argued, that the mere floating of the lungs is no decisive proof of previous life.

With the view of giving this objection its full force, it may be proper first to consider the effects of putrefaction.

Strange as it may appear, it has nevertheless been a subject much debated, what the effects of putrefaction are upon lungs that have never respired; some asserting, that it renders them specifically heavier than water, and consequently, that they will sink when thrown into that fluid; while others, of equal respectability maintain a contrary opinion. Both parties adduce experiments in proof of their particular doctrines. The only solution that can be given to these contradictory results, is by admitting that all the experiments have not been performed with sufficient care, so as to lead to conclusions uniformly just. Every thing depends upon the *manner* in which they are conducted. The most accurate, I believe, were those performed by Mayer, and as they place this subject in a very just point of view, and relieve it of much of the obscurity in which it has been involved, it may not be improper to present a summary of his observations. From a very extended series of experiments, continued during a number of years, and executed with the utmost care and precision, Mayer found, on putting into water the lungs of still-born children, in whom, of course, no sign of respiration or life had appeared, that they sunk to the bottom. After an interval of two or three days, the water in which they were left became turbid; the lungs changed in colour, and increased in volume; here and there an air bubble arose to the surface of the water, and at the same time a putrid odour became perceptible. All these appearances continued to increase daily, until the sixth, seventh, or, at the latest, the eighth day, when the lungs, both entire and cut into pieces, floated in the water in which they became putrid. On transferring the lungs to vessels containing clean water, they still continued to float, although on the slightest compression they instantly sunk.

Lungs placed in water, and exposed to the rays of the sun, swam on the sixth day. If they were suffered to putrefy where there was a free current of air, they rarely floated before the tenth or eleventh day. After the lungs had once floated, they remained in that state, emitting daily a more offensive odour, and acquiring an increased volume, until the twenty-first, or at the latest the thirty-fifth day. After that period they gradually sunk down, without a single exception, to the bottom of the vessel, nor did they afterwards betray any disposition to float, although kept for seven weeks, and in some instances a much greater length of time.*

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* Collectio Opusculorum Selectorum ad Medicinam Forensem spectantium, Curante Dr. J. C. T. Schlegel, Leipsic, 1785–1791, vol. i. p. 262, 3, 4.

The foregoing experiments were made in the month of August. The lungs, both entire and cut into sections, were immersed in pure fountain water, and contained in vessels convenient and capacious. In short, every precaution seems to have been scrupulously observed, to render the experiments accurate and satisfactory.

My own experiments on this subject, although not numerous, go to confirm, in every essential point, those which have just been detailed I shall content myself with recording the history of a single one.

Twenty-four hours after delivery, I was favoured by a medical friend with the thoracic viscera of an infant, which had been still-born, after a very difficult and protracted labour. On putting them into a vessel of water, the heart, lungs, thymus gland, &c. were found to sink readily to the bottom, both conjointly and when separated. No symptom of putrefaction was discernible.

August 20, 9 o'clock A. M. This being the third day after delivery, I separated the two lobes of the lungs, and placed them in a vessel of pure rain-water, exposed to the rays of an intense sun: both lobes being at the bottom of the water.

2 P. M. Both lobes floating. Here and there an air bubble visible on their surface. Odour very offensive.

7 P. M. Left lobe sunk: right still floating.

August 21, 9 A. M. Both lobes sunk.

2 P. M. Both floating.

7 P. M. Both floating.

August 22. Both floating all day.

August 23, 9 A. M. Both floating; covered with bubbles of air. On taking out one of the lobes, with a view of cutting off a portion of it, I found it so far dissolved as to render it impracticable. On returning it into the water, it immediately sunk.

2 P. M. The lung which sunk this morning again floating. Both now float, and are horribly offensive.

August 24, 9 A. M. The lung which was taken out and cut yesterday, sunk again: the other still floating.

August 30. No change has taken place in the situation of the lungs since the 24th. Finding the water nearly evaporated, I filled the vessel very cautiously with fresh rain-water, upon which the floating lung instantly sunk.

August 31. Both lungs still sunk, and never afterwards showed any disposition to float, although suffered to stand for better than five weeks.

From the foregoing experiments it thus appears, that in the *incipient* stage of putrefaction, lungs that have never respired will float in water; whereas they will sink, if it has continued long enough to completely destroy their organization, and thus extricate all the air contained in them. These results have been corroborated by numerous other observations and experiments, and their truth cannot be doubted. It seems singular, indeed, that they should ever have been questioned, when a case perfectly analogous is witnessed in every person that is drowned. The body at first sinks; afterwards rises to the surface, when putrefaction has generated air sufficient to render it specifically lighter than water; and finally descends again, upon the extrication of that air.

Such being the effect of putrefaction. it becomes a question of great importance, to determine in what way we may discriminate between the floating of the lungs, as caused by natural respiration, and that which is the result of decomposition.

Independently of the changes produced in the colour and general appearance of the lungs by putrefaction, there are other very characteristic marks by which they may be distinguished.

1. By the appearance of air bubbles on the surface of the lungs.

On this subject, Dr. William Hunter lays down the following rule: "If the air which is in the lungs be that of respiration, the air bubbles will hardly be visible to the naked eye; but if the air bubbles be large, or if they run in lines along the fissures between the component *lobuli* of the lungs, the air is certainly emphysematous, and not air which had been taken in by breathing."* Jaeger had before this made a similar observation. In lungs floating from putrefaction, he describes the air as contained in the form of bubbles under the external membrane of those organs, where the air introduced by respiration never finds its way.† This rule appears to be founded in truth, and accordingly has been adopted by the best writers on forensic medicine.

2. By the ease with which the air can be extricated from lungs which float in consequence of putrefaction. The evidence of this is to be found in the fact, that if lungs of this description, or any portions of them, be squeezed in the hand, they will immediately sink in water. On the contrary, no compression, however strong, can force out so completely the air from lungs that have respired, as to cause them to sink. This test is insisted upon by Marc, a very distinguished writer on this subject, as the most certain means of discriminating between the effects of putrefaction and respiration.‡

3. By an examination of the other viscera of the body. Numerous observations have established the fact. that with the exception of the bones, the lungs resist putrefaction longer than any other part of the body. Faissole and Champeau, in experiments which they made upon drowned animals, observed that the lungs remained sound, after the whole of the body had become putrefied.§ Mahon noticed the same fact in his dissections of dead bodies.|| Camper ascertained, by direct experiments, that the head became so far decomposed by putrefaction, that the slightest force was sufficient to detach the

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* On the uncertainty of the signs of murder, in the case of bastard children. By William Hunter, M. D. F. R. S. Medical Observations and Inquiries of London, vol. vi. p. 284.

† Schlegel's Collectio Opusculorum, &c. vol. v. p. 111.

‡ Dictionaire des Sciences Medicales. Tom. x. art. Docimasie Pulmonaire.

§ Med. Legal. &c. de P. A. O. Mahon. Tom. ii. p. 400.

|| Ibid, v. 2. p. 400.

bones of it from each other, as well as those of the arms and legs, before the lungs began to participate in the putrefaction.* From these facts the conclusion evidently follows, that if the rest of the body of the child which is the subject of examination, be unaffected by putrefaction, it may very confidently be inferred, that the floating of the lungs is not owing to putrefaction.

By a careful application of the foregoing tests, little or no difficulty can arise in deciding whether the lungs float from putrefaction or from respiration.

But suppose the lungs are found to be actually in a state of putrefaction, is the physician then justified in drawing any conclusions, or in giving any opinion? Mahon advises in such cases, that it is better for the medical witness to be silent, and to leave to the magistrates the task of finding out other grounds of accusation.† Marc, however, answers this question in the affirmative, and proposes two characteristics to enable him to offer a positive decision. The *first* is, that lungs which have respired, notwithstanding they may have been attacked by putrefaction. always have a crepitus when cut into; whereas those which have never respired, although they float in water, are destitute of this peculiarity. The *second*, and which he considers the most decisive and certain, is this: that upon squeezing out from sections of the lungs the matter developed by putrefaction, they will sink if they are from a child born dead; but, on the contrary. if they are from a child born alive, they will, notwithstanding this, continue to float.‡

II. It is objected, that a child may have been born dead, and yet its lungs may float in water, in consequence of their having been artificially inflated, and therefore, it is argued, that the mere floating of the lungs is no proof of previous life.

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* Dissertation on Infanticide, by W. Hutchinson, M. D. London, 1820. p. 47.

† Med. Legal. tom. ii. p. 400.

‡ Manuel D'Autopsie Cadaverique, Med. Leg.

It has been doubted by some whether artificial inflation of the lungs could ever be effected. Heister states, that he proved by actual experiments, that air cannot be blown into the lungs so as to cause them to float.* Hebenstreit also doubts whether it can be accomplished, in consequence of the mucus which is usually found to fill the fauces of a new-born child.† Roederer, from the failure of his experiments on this subject, was led to the conclusion, that it can only be effected after the child has previously breathed. "A spiritu ori," says he, "inflato pulmones infantis non inflari dilatarique; nisi fœtus aliunde respiraverit."‡ Brendel is still more positive on this point. He believes artificial inflation to be utterly impossible, and assigns two reasons for his scepticism. The first is, the resistance which is made by the thorax and diaphragm, and the second is, the difficulty of introducing a pipe into the glottis, without which he thinks it is impossible to inflate the lungs. He adds, moreover, in confirmation of his opinion, that he made experiments upon pups that were killed while yet in the uterus, and although he attempted to force in the air by a bellows, yet no change was effected upon the lungs, and they sunk when put into water.§

A contrary doctrine is however maintained by a very large majority of the most respectable authorities in forensic medicine. Low admits the possibility of it, and tells us that Bohn, together with the medical faculty of Leipsic, concurred in the same opinion.‖ Ludwig says, it is certain that air may be artificially blown into lungs which have never respired, and that they will afterwards float in water.¶ In several experiments made to test this matter by the celebrated Camper, the result was uniformly in favour of this opinion.* Jaeger, Buttner, and Schmitt, concur in the same, as do most of the French and English writers.

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* Morgagni's Works. vol. i. p. 536.

† D. J. Ern. Hebenstreit Anthropologia Forensis, &c. p. 405. Leipsic, 1751.

‡ Schlegel's Collectio Opusculorum, &c. vol. v. p. 112.

§ Prælectiones Academicæ, J. G. Brendelii in H. F. Teichmeyeri Institutiones Medicinæ Legalis vel Forensis, &c. p. 186. Hanover, 1787.

‖ Johannis F. Low, Theatrum Medico Juridicum, &c. cap. xii. p. 623. Norimbergæ, 1725.

¶ D. C. G. Ludwig, Institutiones Medicinæ Forensis Prælectionibus academicis accommodatæ, p. 97. Leipsic, 1765.

From the foregoing detail of authorities it is quite evident, that although artificial inflation of the lungs of a child born dead, is a thing perfectly practicable, yet it is not accomplished with as much facility as many have imagined. This consideration I conceive to be important, because it tends to weaken very much the force of this objection to the hydrostatic test. Still, however, the objection holds good, and there are not wanting occasions when artificial inflation might be attempted. It is not incredible that it might be the result of malice, designed to injure the innocent mother; or of maternal tenderness endeavouring to resuscitate a lifeless child. It becomes, then, a matter of great moment, to determine whether the existence of air in the lungs be the product of nature or of art: and it is fortunate for the cause of justice, as well as humanity, that this can be done.

The first test which I shall notice for this purpose, was originally proposed, I believe, by Buttner, and is founded upon the difference of the fœtal and adult circulation of the blood. In the former, it is well known that the blood does not pass through the lungs; whereas, as soon as respiration commences, the old passages are closed, and the whole mass is forced through those organs. If, therefore, a child has been born dead, the arteries and veins of the lungs are found destitute of blood, and in a collapsed state, notwithstanding any artificial inflation that may have been practised upon them. On the contrary, the vascular distention of the pulmonary organs, proves that the child has breathed; for nothing but natural respiration can produce this effect.

A second method of determining this question is, by taking the weight of the lungs, and comparing it with the weight of the whole body, according to the celebrated test of M. Ploucquet. As this is a test of great value, it will be necessary briefly to explain the principle upon which it is founded.

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* Schlegel's Collectio Opusculorum, &c. vol. v. p. 112.

From the peculiarity of the vascular system in the fœtus, only a very small portion of the blood goes the round of the pulmonary circulation. As soon as respiration commences, a change is effected, and the whole mass of the blood passes through the lungs, in order to undergo the necessary process of oxygenation. From this it appears obvious, that the fœtal lungs must be considerably inferior in weight to the same organs after respiration has been established. It is upon this fact that M. Ploucquet founded his test for determining, whether a child had been born dead or alive, by comparing the weight of the lungs with the weight of the whole body. From the experiments which he made to ascertain their proportional gravity, he drew the general conclusion, that the weight of the lungs before respiration, is one-seventieth of the weight of the whole body; whilst after respiration has commenced, it amounts to one-thirty-fifth; or, in other words, that the blood introduced into the lungs in consequence of respiration, *doubles* their absolute weight.

A third test has been suggested by M. Marc. He considers that art can never completely inflate the lungs; and, from the greater difficulty which attends the admission of air into the *left* lung, he is induced to believe, that the inferior extremity of that lung will remain in a collapsed state, and float but imperfectly, or not at all.* This test is certainly ingenious, but, I think, quite inconclusive, inasmuch as the same appearances may be observed after natural respiration has taken place; and if so, it can furnish no ground of distinction between respiration and artificial inflation of the lungs. It is a curious fact connected with the history of incipient respiration, that the *right* lung receives air much sooner than the left. M. Portal first discovered this, and afterwards confirmed it by numerous experiments. In a kitten, which he killed a few minutes after it was born, the right lung was of a whitish colour, filled the whole cavity of the chest, and swam in water. The left was of a dark red colour, in a collapsed state, and sunk in water. He accounts for this interesting phenomenon, by showing that there is a difference in the size and direction of the bronchiæ leading to the two lobes. Upon examination, he found the right one about one-fourth part thicker, and one-fifth shorter, than the left; besides, he found the passage to the right to be more direct than that to the left.*

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* Manuel D'Autopsie Cadavérique, &c. p. 138.

III. It has been objected, that "a child will very commonly breathe as soon as its mouth is born, or protruded from its mother; and in that case, may lose its life before it is born, especially when there happens to be a considerable interval of time between what we may call the birth of the child's head and the protrusion of the body."†

This objection did not originate with Dr. Hunter. It is noticed by Morgagni; and I find it discussed by the German writers early in the last century. It must be admitted, however, that the high authority of Hunter's name, has given to it an importance which it otherwise would never have possessed; and it is on this account more especially deserving of examination. It involves two points, each of which is worthy of distinct elucidation. Is it possible that a child can breathe, when nothing more than its head is delivered? and if so, is it probable that after having respired in this situation, it will die before the delivery of the rest of the body?

Although it is denied by some very respectable authors, that a child can perform the act of respiration when merely its head is born, yet the fact rests upon evidence too substantial to be contradicted. Independently of the authority of Dr. Hunter, we have several other writers who furnish us with decisive testimony on this subject. Marc alludes to a case of this kind reported by M. Siebold.‡ Capuron, a respectable French writer on Legal Medicine,* relates a similar instance which occurred in his own practice. And Osiander informs us, that he had witnessed twelve cases in which the child breathed and cried as soon as the head was born.† It must therefore be conceded, that a child may breathe and cry as soon as its head is delivered, although it is equally true that it is by no means a common occurrence. Admitting, then, that a child may actually breathe in the situation we have supposed, is it probable that it will lose its life before the complete expulsion of the body? That it is not, appears to me of very easy demonstration; and if so, the objection loses at once almost all its force. Even among the writers who are most strenuous in support of this objection, I have not met with a single one who pretends to have witnessed an instance in which a child has actually died in this situation. Low, although he thinks it possible, relates no case of it. Dr. Hunter, whose professed object was to enforce all the probable exceptions to the hydrostatic test, gives us nothing more than his opinion, unsupported by facts. Mahon barely admits the possibility of it. Capuron, who is sufficiently sceptical on this subject, contents himself with recording the case already alluded to, in which the child was safely delivered. Even Osiander, with all his marvellous cases, does not present us with a single one of this kind. In point of fact, therefore, there is no instance recorded, so far as my knowledge extends, in which a child has actually expired under these circumstances. This, however, does not prove that it might not occur; and it is therefore necessary to inquire into all the possible causes which might produce its death. If a child expires after the delivery of the head, and before the expulsion of the rest of the body, its death will probably be owing to one or other of the following causes:— 1. Natural debility of the child. 2. Pressure on the umbilical cord, interrupting the fœtal circulation. 3. Cessation of labour pains. 4. Unusual shortness of the umbilical cord. 5. A preternatural enlargement of the *body* of the child. 6. A tumour upon some part of the body of the child, mechanically interrupting parturition. I shall very briefly examine each of these in order.

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* Duncan's Medical Commentaries, vol. i. p. 245. Am. ed.

† Dr. W. Hunter, in the Medical Observations and Inquiries of London, vol. vi. p. 287.

‡ Manuel D'Autopsie Cadavérique, p. 140.

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* La Med. Legal, relative a l' art des Accouchmens. Par J. Capuron, p. 405.

† New-York Medical and Physical Journal, vol. i. p. 372.

That *natural debility* on the part of the child cannot occasion it, seems to be proved by the very fact of respiration having taken place; for the exercise of that function so prematurely, necessarily implies a degree of vigour inconsistent with the supposition of such original feebleness.

That *pressure on the cord* should produce the death of the child, appears equally improbable. It is perfectly plain, that when this cause proves detrimental, it must be anterior to respiration, and when as yet the life of the child depends wholly upon the fœtal circulation. In the present instance, however, the child is supposed to have already breathed; and therefore any accidental interruption in the fœtal circulation cannot, in all probability, be attended with any injurious consequences.

Cessation of the labour pains. If, after the delivery of the head, there be a sudden cessation of the pains, there is no doubt that the child may be retained in this awkward situation for some time, and that it may even lose its life before it is completely expelled. Still it must be obvious, that the chance of such an issue is very much diminished in all those cases where respiration has actually commenced, inasmuch as the performance of this function proves not merely that the child is vigorous, but also that its thorax and body are not so closely compressed by the parts of the mother as to endanger its life. Hence a child, under these circumstances, may be detained a considerable length of time, without jeopardizing its existence.

Unusual shortness of the cord. Cases of this kind occasionally occur. But here too the very fact of respiration having commenced, gives the child the best possible chance of being eventually born alive.

Preternatural enlargement of the body of the child, more especially of the shoulders, may prevent the delivery of the child, even after the birth of the head. That a child might die from this cause, is not disputed; but the very fact of its shoulders and chest being so large as to prevent delivery, shows how difficult, if not impossible it would be, for it to respire. If, however, it did actually respire, then the hazard of a long detention in this situation, would, by this very circumstance, be materially diminished. In addition to all this, the cause would here be so very obvious, on a bare inspection of the child, that no serious error could possibly arise from this source.

A tumour on the body of the child. This, of course, must be a very rare occurrence, and can never lead to any false decisions. I mention it merely because a case of this kind is recorded, in which "the head of the child was protruded, and the expulsion of the body, for a considerable time, prevented, in consequence of a large excrescence on the left breast of the child. During this interval, which was about half an hour, the child frequently cried so loud, as to be heard by the attendants."* It does not, however, appear even in this case, that the child eventually lost its life; at least, nothing is stated to this effect, in the account which is given of it. So far from supporting the objection of Dr. Hunter, which we are considering, it proves, in the most pointed and satisfactory manner, how little danger attends the child in this situation, when it enjoys the benefit of respiration.

From the foregoing discussion, it may, therefore, fairly be concluded, that, in reality, very little danger attends the child under the circumstances which we have supposed.

I shall sustain this argument by the opinions of one or two writers, distinguished for their extensive experience, as well as practical sagacity. In a case of this kind, Burns directs that we should "attend to the head, examining that the membranes do not cover the mouth, but that the child be enabled to breathe, should the circulation in the cord be obstructed. *There is no danger in delay,* and rashly pulling away the child is apt to produce flooding, and other dangerous accidents." In another place he says, "some children die, owing to the head being born covered with the membranes sometime before the body. This is the consequence of inattention, for if the membranes be removed from the face, there is *no risk of the child*."* Denman also remarks, that "it was formerly supposed necessary for the practitioner to extract the body of the child immediately after the expulsion of the head, lest it should be destroyed by confinement in this untoward position. But experience has not only proved that the child is not, on that account, in *any particular danger, but that it is really safer and better, both for the mother and child,* to wait for the return of pains, by which it will soon be expelled; and a more favourable exclusion of the placenta, will also, by this means, be obtained."†

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* Mahon's Essay on Infanticide, translated by Christopher Johnson, of Lancaster.—See note by Mr. Johnson, p. 25.

On a review of the whole of this part of our subject, it results, that a child may occasionally breathe as soon as its head is delivered: that the very fact of its breathing in this situation, gives it the best possible chance of being born alive; and, finally, if it should even die, the cause of its death will generally be at once evident upon a mere examination of the body of the infant.

From the preceding examination of objections to the *hydrostatic* test, I think we may safely come to the following conclusions:

1. That when the lungs float in water, it must be from one of three causes: natural respiration, putrefaction, or the artificial introduction of air.

2. As the lungs may float from other causes beside respira-

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* The Principles of Midwifery. By J. Burns. Ed. by Dr. Chapman, p. 246, 376.

† An Introduction to the Practice of Midwifery. By T. Denman. Am. Ed. by J. W. Francis, M. D. p. 289.

tion, their mere floating is no proof that the child was born alive.

3. As, however, it is possible to discriminate between the floating of natural respiration and of that which is the result of other causes, it follows,

4. That, with due precautions, the floating of the lungs may be depended upon as a safe and certain test, that the child has been born alive.

I shall now proceed to examine another class of objections to the hydrostatic test, which have been much insisted upon, and which have tended, perhaps, no less than those which have already been considered, to shake the faith of the public with regard to its accuracy.

I. It has been objected that the child may have breathed, and yet the lungs, in consequence of diseases of various kinds, may have their specific gravity so increased as to cause them to sink in water. And, therefore, it is argued that the sinking of the lungs is no proof that the child was not born alive.

This objection has been deduced principally from analogy. It has been observed, that various morbid affections of the pulmonary organs of adults, as peripneumony, hydrothorax, calculi, schirri, ulcers, &c., will cause their subsidence in water; and hence it has been inferred, that the same might take place in the fœtus.

That the lungs of adults may undergo such changes from disease, as to cause them to sink in water, I am not disposed to deny, although I think it occurs less frequently than is generally believed. Haller informs us, that he has seen lungs of this description, which nevertheless continued to float. "Vidi sanguinem ex ruptis per funestam peripneumoniam vasis in pulmonem effusum, ut tamen nataret; vidi schirrhos, calculos, et lymphaticum coagulum, ut minime tamen subsideret."* He admits, however, having seen a case of peripneumony, in which the lungs sunk in water; and he adduces several other cases of a similar nature upon the authority of others.* Heister records the case of a young man who died of phthisis, whose lungs were specifically heavier than water.† De Haen relates, that he met with three or four cases, in which portions of the lungs of adults sunk in water: and, he adds, that Diemerbroeck witnessed the same in a patient whose lungs were in a schirrous condition.‡ Hoffman details the case of a young man who died of pneumonic fever, in whose lungs the same was observed.§ In speaking of the dissections of those who died of pleurisy, in the Island of Minorca, Cleghorn says, that "in many the lungs were converted into a hard liver-like substance, and *sunk in water*."‖ Dr. Baillie confirms the same general fact. "In consequence," says he, "of the greater quantity of blood being accumulated in the inflamed portion of the lungs, they become considerably heavier, and will *frequently sink in water*."¶ Finally, there is recorded, in a late Journal, a case of "condensed lungs," occurring in a subject twenty-four years of age, and which immediately sunk on being immersed in water.** These facts are sufficient to illustrate the grounds upon which the objection rests. And they prove incontestibly, that the lungs may occasionally be rendered, by disease, specifically heavier than water. It cannot be admitted, however, that these morbid conditions will frequently occur in the fœtus, for it is not exposed to the influence of the causes which usually produce them. Haller, notwithstanding his great experience and extensive learning, relates no instance of it, and expressly asserts, that they are very rarely found in the fœtal state. "In adulto homine *aliquando*, in ſetu *rarissime*, ut pulmo calculis, schirris, aliave materie, morbose gravis in aqua subsideat, etsiquam respiraverit."* Brendel, in speaking on this subject, relates only a single case of an abortive fœtus which had schirrous lungs, and considers it a singular occurrence.† I shall only add, in confirmation on this point, the opinion of Dr. Duncan, the accomplished editor of the Edinburgh Medical and Surgical Journal. "Unquestionably a piece of inflamed lung will sink in water like a piece of liver, *but we doubt that such inflammation was ever observed in the lungs of a new-born infant*, concerning which a question of its having been still-born could arise; and we deny the fact that any portion of lungs which have breathed, will ever be rendered specifically heavier than water, by the mere settling of the blood in the lower portions after death."‡

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* Elem. Physiol. Tom. iii. p. 281.

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* Ibid.

† Hebenstreit, Anthropologia Forensis, &c. p. 401.

‡ Ratio Medendi, &c. p. 114.

§ Opera Omnia, tom. 2. p. 140.

‖ Observations on the Epidemical Diseases of Minorca. By George Cleghorn, p. 159. Am. Ed.

¶ Morbid Anatomy, p. 38. Am. Ed.

** Edinburgh Med. and Surg. Journal, vol. iv. p. 301.

It appears then, as well from reason as from facts, that the objection is founded upon the existence of circumstances barely possible, and by no means probable; as such, however, it demands consideration, and it is necessary to suggest the means by which a false judgment may be prevented.

For this purpose we have a test both simple and certain. The objection takes it for granted that the child has breathed; whether feebly or vigorously is a matter of no consequence. Some part, therefore, of the lungs *must* contain air, and although the quantity of it may be too small to cause the whole lungs to float, yet if they be divided into a number of pieces, and any of them remain on the surface, there cannot be a moment's hesitation about the conclusion to be drawn. Foderé states that he frequently made experiments upon lungs that were schirrous, or had congestions of blood, and he uniformly found, that although they sunk when put into water entire, yet, when cut into pieces, some of them always floated.§

II. It has been objected, that a child may have actually breathed, but yet so feebly and imperfectly, that the lungs shall not have received air sufficient to enable them to float: and hence it is argued, that the sinking of the lungs is no proof that the child was born ~~alive.~~ Dead

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* Element. Physiol. tom. iii. p. 281.

† J. G. Brendelii, &c. Medicina Legalis sive Forensis, p. 10.

‡ Edinb. Med. and Surg. Journal. vol. xii. p. 79, 80.

§ Med. Legal. tom. iv. p. 487.

In support of this objection, facts of a very pointed nature have been adduced. Heister relates the case of a very feeble infant, whose lungs sunk in water, though it lived nine hours after birth.* And a late writer on Infanticide states, that he had been informed by a physician to the Foundling Hospital at Naples, who opened daily, on an average, the bodies of ten or twelve infants, which had generally died within twenty-four hours after birth, that he hardly ever found more than a very small portion of the lungs dilated by air: this portion was frequently not larger than a walnut in its green shell, and but rarely larger than a hen's egg, and it was commonly situate in the right lung.†

The same method must be here adopted, as in cases where the lungs are diseased: they must be cut into several parts, and experiments instituted upon each. However imperfect the respiration has been, some portion of the lungs will necessarily be inflated, and therefore must ~~sink~~ float.

III. It has been objected, that a child may be born alive without breathing; and therefore, in these cases, the sinking of the lungs is no proof that the child was not born alive.

The fact upon which this objection is raised, cannot be questioned; nevertheless it is both safe and just to consider as dead every child that has not breathed. Governed by such a rule, any error that may be committed will always be on the side of mercy. It is true, that certainty is as desirable here as in any other case; for the destruction of a feeble child is a crime as enormous as that of a vigorous and healthy one; and the punishment of the murderer of the one, is equally an object of public concern with that of the other. But, in the language of a distinguished writer on this subject, "pour le punir, il faut le constater; et lorsque les limutes de l'art nous refusent le degré de certitude que [illegible] ambitionnons, la clémence, que dis-je, la crainte d'immoler l'innocence devra l'emporter sur toute autre consideration."*

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* Morgagni's Works, vol. i. Lett. xix. p. 586.

† A Dissertation on Infanticide, in its relations to Physiology and Jurisprudence. By W. Hutchinson, M. D. 1820.

This objection, so far from showing the inconclusiveness of this test, serves only to establish more clearly its absolute necessity. It is by resorting to it alone, that the sacrifice of innocence can be prevented: for who would assume the responsibility of deciding that a child had been born alive, when no evidence could be discovered of its having respired?

From the foregoing considerations, it may therefore be concluded,

1. That when the lungs sink in water, it must be from one or other of the following causes: the total want of respiration; feeble and imperfect respiration; some disease of the lungs rendering them specifically heavier than water.

2. As the lungs may sink from other causes than the absence of respiration, their *mere sinking* is no decisive proof of the child's having been born dead.

3. As, however, the sinking from the want of respiration may easily be distinguished from that which is the result of other causes, it follows,

4. That with due precautions, the sinking of the lungs is a safe test that the child was not born alive.

I have now gone through the discussion of this subject; and although the general conclusion is decidedly in favour of the accuracy of the hydrostatic test, yet nothing can be plainer than the necessity of an extensive acquaintance with the subject, to enable the professional witness to do justice to himself and to the cause of truth. It is much to be feared, that from the ignorance of some, and the precipitancy of others, great and fatal errors have not unfrequently been committed. It may not, therefore, be improper to present a summary of *practical rules*, for the guidance of the physician when called to the examination of a case, which, of all others, demands a combination of the exercise of the soundest judgment, and the most profound knowledge.

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* Manuel D'Autopsie Cadevérique, par C. C. H. Marc, p. 140.

1. As preliminary to any examination of the lungs, the child should be weighed, and the general appearance and condition of the body should be particularly noted, with the view of ascertaining the following points, viz. : if the child be full grown ; if the different parts of its body be well proportioned ; if the shoulders be uncommonly large, when compared with the size of the head ; if any tumours are to be found upon the body; if the cord be unusually short; and, finally, if any symptoms of putrefaction be present.

2. The chest should then be carefully opened, and the following things noticed : the general shape of the thorax, whether it be much arched or otherwise ; if the lungs be collapsed or dilated ; whether they cover the lateral parts of the pericardium and heart ; whether their colour be deep red or lighter ; whether there be any appearance of disease or of putrefaction.

3. The next step is to remove the contents of the chest, for the purpose of performing the necessary experiments upon the lungs. The aorta and vena cava should first be tied near the heart, and then cut beyond the ligatures : the trachea should then be also divided : the lungs, together with the heart, are now to be taken out of the chest, and to be submitted to an additional inspection, to ascertain whether they are sound or diseased, and if they are at all affected by putrefaction.

4. A convenient vessel containing water should now be provided ; and particular attention should be paid to the temperature of the water, in which the lungs are to be immersed. The reason of this will be perfectly obvious, when it is recollected, that the specific gravity of water varies with its temperature : thus, for instance, water at 100° is lighter than water at 60°, and still lighter than at 40°. Besides, if the water be too hot, it will have the effect of expanding the lungs, and thus favour their floating, especially when there already exists a tendency to putrefaction. If, on the contrary, its temperature be too low, the air cells may be contracted, and much of the air be

thus expelled. The temperature of the water should therefore be regulated by that of the surrounding air. Another precaution relative to the water is, that it should not be impregnated with *salt ;* for, in consequence of the greater specific gravity of saline water, a body might float in it, which would sink in fresh water.

5. The lungs, together with the heart, should then be cautiously placed in the water, and it should be observed whether they float or sink : if they float, whether above the surface of the water, or just under it ; if they sink, whether they do so rapidly or gradually.

6. The lungs should then be taken out of the water, and after tying the pulmonary vessels, they should be separated from the heart, and accurately weighed.

7. The lungs should then be replaced in the water, to see whether they sink or float, and in what way.

8. The two lobes should then be separated, and the same experiment repeated upon each, noticing the difference, if any, between them. If only one floats, see if it be the *right* one.

9. Each lobe should then be divided into a number of pieces, taking care not to confound the fragments of one lobe with those of the other ; and upon each of these the same experiments should be instituted.

10. While cutting the lungs, observe if there be any crepitus ; if the vessels are charged with blood ; and if there be any traces of disease.

11. If any of the sections of the lungs float, they should be taken and squeezed forcibly in the hand, and then replaced in the water, to determine whether after this they will sink.

After having performed these different processes, the conclusions to be drawn from them are evident. If there is nothing to be discovered on the body of the child, to favour the belief that it might have lost its life during delivery ; if the lungs be not touched by putrefaction, nor be artificially inflated ; if on cutting into them, a crepitus be perceptible ; if the entire lungs, as well as all the separate divisions of them, remain on the surface of the water ; if, after squeezing portions of the lungs.

they still continue to float: then the mass of evidence is irresistible, that the infant was born alive, and enjoyed perfect respiration. If only the *right* lung, or its pieces, float, the respiration has been less perfect. If some pieces only float, while the greater number sink, it proves respiration to have been still less complete. On the other hand, if neither the entire lungs, nor any section of them, float in water, the inference is decisive, that the child never respired. Several other inferences of value are to be deduced from the foregoing experiments; but as these must be perfectly obvious, after what has been stated in the former part of this paper, it is deemed unnecessary to enter into any farther details.

ART. VII. A CASE of ABORTION, *at the sixth month, with attachment of the Placenta over the os uteri.* Communicated to Professor ANDERSON, of this city, *by* ROBERT BAYARD, M. D. of Horton, Nova Scotia.

MY DEAR SIR,

Enclosed you have the summary of a case that occurred to me in practice, which, should you think worth a place in any of your periodical journals, you will please to have it inserted.

Among the various perplexities which occur in obstetric practice, there is none that has excited greater alarm and embarrassment, than the attachment of the placenta over the os uteri. The hemorrhage which follows the parturient action of the uterus, has occasioned a promptitude of practice, which, notwithstanding the apparent danger, seems to have originated more from apprehension than the actual urgency of the case, when anatomically considered. It is true, the danger is great too great, to admit of quietly awaiting the efforts of nature. Such indifference would prove fatal to the patient, in the great, majority of cases, as instances have occurred as exceptions, in which the child and placenta have, under such an attachment, been both expelled, and the woman survived; not, however, without a loss of blood, which induced an alarming exhaustion; but, it must be observed, that in the instances of this kind, which came within my own knowledge, the women were unfortunately attended by unskilful practitioners, who were not aware of the proper means of restraining the violence of the hemorrhage.

It has been generally recommended, when the placenta is implanted over the os uteri, to expedite labour; and, as soon as the os uteri is partially dilated, to force the hand through it, lacerate the placenta, rupture the membranes, bring down the feet of the child, and effect delivery as speedily as possible. Now, I object to this, in toto, as a general rule of practice; and, I suspect, that, in many cases, it would increase the difficulty and danger. It is certain that it always increases the sufferings of the woman, and is seldom, if ever effected, without a most distressing loss of blood. I have delivered patients by this violent interference, and have witnessed scenes of anguish, flooding, and debility, which imperiously point out the necessity of ameliorating the practice in such deplorable cases. Anxious to effect an improvement so essential, I was determined to adopt a different mode of procedure, in the first case that should come under my charge. The following observations, I tru t, will induce other practitioners to repeat proceedings which were successful, and seem justified by the anatomical arrangement of the parts.

Mrs. Ebenezer K——n, the wife of a respectable farmer, and the mother of a large family, was subject to frequent miscarriages about the fourth month; which had induced a leucophlegmatic appearance, and general as well as uterine debility. She was particularly disposed to menorrhagia, especially at the first monthly period after her confinement, whether regular or premature; she suffered severely from excessive flowing I was called to attend her in the beginning of January, when I found her losing blood freely from the os uteri; she was then four months pregnant; complained of little pain, but more of a sensation of uneasiness across the hips; there was no expulsive effort of the uterus; the bowels were costive; the countenance pale; the pulse feeble; the sanguineous discharge from the uterus was reduced in quality by a copious admixture of water, which the patient referred at first to the rupture of the membranes. The bladder was but slightly affected. From the appearance of symptoms, I was disposed to attribute the discharge partly to over exertion on the part of the patient, who was an industrious and hard-working woman, but principally to a morbid habit of the womb, induced by previous miscarriages. I accordingly enjoined strict rest, the recumbent posture, and gave liberal opiates, the gentlest locatives, and ordered the use of cold astringent applications, with the employment of plugs, as recommended in early abortions. This plan was pursued for several days, during which the discharge continued. This was however unquestionably owing to the very imperfect manner in which the vagina was plugged. The patient in a few days became somewhat feverish, which I ascribed principally to a costive habit of body, but fearing there might be some uterine congestion, I determined, in spite of the contra-indicating appearance of her countenance, and weakness of pulse, to draw some blood, which I did; but a few ounces satisfied me of the inutility of it. The first plan was therefore more rigorously enjoined. I examined the os uteri in the first instance and found it not sufficiently opened to admit the end of the finger. As I could not pass it within the womb, I was disposed to attribute the discharge to congestion and the rupture of some vessels about the cervix or os uteri. As the patient could but poorly sustain a further loss of blood, cold astringent injections were thrown up, and the plug again introduced; this seemed to do well for a few days. The debility of the woman was so great as to demand moderate cordials and tonics; the camphoretted emulsion, the tincture of cinchona, and moderate port wine were accordingly allowed; her countenance and strength seemed evidently improved by them. The discharge of blood was less, and its watery proportion greatly reduced; the discharge at times had an unpleasant smell, which led the woman and her friends to suspect that the child was dead. But the patient soon changed her opinion from a consciousness of the movement of the fœtus in utero. The copiousness of watery admixture with the blood was such for four or five weeks, as to excite reasonable suspicions of an hydropic affection of the womb; which seemed strengthened by the general appearance of the woman. Eight weeks elapsed with occasional alternation of symptoms.

February 26th—I was sent for, and found her labouring under manifest indications of abortion: she had lost much blood during the night; her pulse was very quick and feeble; she was faint, and had occasional imperfect labour pains, which were characterized by a sensation of anguish, rather than of bearing down. I examined the os uteri, and found it dilated in diameter about two-thirds of an inch, and perceived the placenta implanted over it; the vagina was filled with coagula. I could pass my finger within the os uteri, and satisfactorily feel the adhesion of the placenta to the uterine surface. from which it was detached about an inch all around the os uteri. I was now sensible the discharge came from this divided portion: whether it issued equally from the placental and uterine vessels, I am not prepared to say. I was aware that no blood could escape into the uterus, which was not only already filled completely with its fœtal contents, but was guarded from such an influx by the yet adhering placenta. I was also sure, that if the vagina was thoroughly secured, and its cavity plugged, that very little blood could be lost in that cavity: my chief desire was to prevent the effusion of blood, and promote the expulsitory efforts of the womb. I considered, that the further loss of blood to any considerable amount in the reduced state of my patient. would have been fatal to her: and I also anticipated some difficulty in forcing the womb at this period of gestation. I accordingly plugged the vagina, and kept an attendant pressing against the labia vaginæ with a soft towel folded as a compress, so as effectually to prevent the discharge of any blood. I gave cordials; and by friction on the abdomen promoted, as far as I could, uterine contraction: the pains were now slight: three hours passed in this manner, during which there was little or no discharge, excepting when I introduced the finger to ascertain the progress of the labour. My patient did not grow sensibly weaker. In the course of four hours, the efforts of the womb became more frequent and strong, so that I found the detached placental portion protruding through the os uteri; while I at the same time could distinctly ascertain its adhesion within the uterus, now about a finger's length from the dilated margin: as the portion protruded, I found I could insinuate my finger into its lobulated texture without lacerating afresh any more vessels, and plainly feel its membraneous lining. I now kept my finger constantly within the vagina, favouring as much as possible the dilatation of the os uteri. I at the same time guarded against the hemorrhagy, which I found would otherwise ensue, by compressing the labia vaginæ *firmly* together with my thumb and other fingers, whilst the index finger was within the vagina. This was easily and advantageously managed. The pains now assumed a determined character; and, as they were increasing, I watched my opportunity and ruptured the membrane; the liquor amnii gushed out copiously; a strong contraction took place; the volume of the uterus was diminished; the disposition to hemorrhagy abated; the placenta passed into the vagina, and in a few minutes the fœtus was expelled, completely enveloped with its placenta. The disposition to flooding of course ceased; and my patient was delivered with comparatively little pain, and very little discharge from the time the plugs and compress were firmly employed. It may possibly be observed, that the practice adopted in the foregoing case might answer in premature labour, but be inapplicable at the full period, when the blood-vessels are much larger, and the hemorrhage consequently more copious. In reply, I must remark, that in the above instance the placenta was unusually large, and the circumstances altogether of such a nature, as to convince me of the expediency of repeating the same means at any period, until careful experience invalidates or condemns them. If we advert to the relative thickness of the uterine parietes at the sixth month, and full period of gestation, the danger from hemorrhage, *cæteris paribus*, would be most urgent at the sixth month; because, at the ninth month, the substance of the uterus is thickest, and when contraction occurs, especially after the liquor amnii is evacuated, it allows the divided vessels to shrink into that substance: moreover, the pains are generally more powerful at this period, and the muscular efforts of the womb compress its lacerated vessels more forcibly, and thereby lessen the tendency to flooding. If strong pains, and increased thickness of the parietes of the womb, are favourable in repressing hemorrhage, weaker pains, and proportionate thinness of texture, must conduce to an increased discharge. That the parieties of the womb are thinner at the sixth than at the ninth month, no person will deny: and if the force of contraction in this case, is in the ratio of the size of the muscle, which I think is also indisputable. the uterine vessels are less liable to be compressed, and are, of course, more disposed to copious bleeding. To this imperfect contraction of the womb, and consequent diminished compression of its vessels, I think we may frequently refer the disproportionate hemorrhage which occurs in the early months, when a large quantity of blood flows freely from a comparatively small surface. It may be said, that a woman suffers pains as strong in the early months of miscarriage, as she does in regular labour. I can only say, that in a variety of cases which have come under my charge, I have never observed them. I admit, that she often suffers more from abortion than regular labour. But the suffering is not attributable to the strength of the pains: it is anguish, referable rather to the occurrence of a process in an irritable or irritated constitution, unprepared by nature for any such morbid action. I have now to request of you, or any of your friends, a candid opinion, with remarks, on the subject of the above case.

Yours,

ROBERT BAYARD.

ART. VIII. DIARY *of the Occurrences of the first month of the* YELLOW FEVER *which prevailed in the City of New-York in* 1822, *with facts and observations relative to the nature and character of the disease.* By DANIEL D. WALTERS, M. D. of New-York.

New-York, Dec. 17, 1822.

TO THE EDITORS OF THE
NEW-YORK MEDICAL AND PHYSICAL JOURNAL.

AWARE of the deep solicitude of the public on all that concerns the late pestilence, I have offered for publication in your Journal a detailed account of such facts as may illustrate the origin and nature of the disease. Less minuteness would have defeated the object I have in view. The present paper is intended to be submitted to the consideration of the Board of Health of our city.

With sentiments of respect,
I remain,
D. D. WALTERS.

To Drs. FRANCIS and BECK.

TO THE
HONOURABLE STEPHEN ALLEN, ESQ.
MAYOR OF THE CITY OF NEW-YORK, AND PRESIDENT OF THE BOARD OF HEALTH.

SIR,

I have written a Diary of the first month of the late fever, as it appeared in this city, with some facts and observations relating to the disease. Besides attempting to show whence the fever of this year originated, I had other objects in view. One of these was to throw in my mite towards the correction of public opinion on this subject; another was to prove that the Board of Health, as now appointed, is not organized on the best possible plan, and that the laws which regulate it, as well as those governing the quarantine establishment, are partly inadequate to the object, and more or less founded on erroneous principles. Another object to be answered by this communication, was to show that although the Board of

Health could not be officially informed of the existence of pestilence until a month after its appearance; yet that some persons did know the true character of the disease at its beginning. If this had been the first occurrence of the kind which had fallen under my own observation, I should most likely have remained silent on the subject. The same difficulties have happened in a greater or less degree several times before, and are even likely still to occur, until some radical change takes place in the organization of the Board of Health, and in the laws regulating the quarantine establishment.

Since the year 1798, I have thought a communication of the kind here presented, due to the public interest: for in that year the same difficulties took place, with consequences shocking to humanity, and which beset the Board in so remarkable a manner during the first month of the late fever. The Board of that year, while feeling no doubt the most laudable concern for the sufferings of their fellow-citizens, and actuated by the best motives for the public good, could not become officially informed of the existence of pestilence in the city, until its ravages had spread far and wide beyond its control: its exertions were therefore of course paralyzed, and it did nothing except passing a resolve, for quieting the great alarm which existed at that time, "*That the city was free from yellow fever.*"

In order to bring the whole ground of difficulty before the public in the way best calculated for its correction, I have thought it best to proceed in the manner I have done, that is, to give a detail of all the facts as they occurred daily, in connexion with my own relative proceedings, and for whatever may appear to savour of egotism, my only apology is, that the public good seemed to require it. These sheets I now beg leave to submit to you, and through you to the gentlemen who compose the Board of Health; because I have used some of their names with a little freedom. I have quoted some conversations which took place between them and myself. If I have misquoted any thing, I hope they will be so kind as to correct it, for with that intention it is laid before them; and if by any thing that I have written, the interests of this city can be promoted, I shall consider my feeble exertions to be well rewarded.

I have, Sir, the honour to be
Your most obedient,
and humble servant,
DANIEL D. WALTERS.

New-York, Dec. 17, 1822.

On the evening of Thursday, the 11th of July, 1822, I visited Mr. Reder's daughters, Amanda and Caroline, the former aged eleven, and the latter nine years. I found them both labouring under the general symptoms of a violent fever. Their mother informed me that they had sickened on the preceding evening. Mr. Reder resides in a neat, airy, brick house, two stories high, situated at the west end of and fronting Rector-street, one door from Washington-street. This spot, in common with all the west side of the city, was always, until the present season, justly considered remarkably healthy, and, indeed, its local advantages in favour of health are certainly very great, as it lies entirely open to the cooling and ventilating operation of the southerly and westerly winds which prevail nine-tenths of the time during our summer months.

Friday the 12th.—I found my patients without any abatement of fever.

Saturday the 13th, was a rainy day, and being myself indisposed, I therefore did not visit them.

Sunday the 14th.—I saw them at 10 o'clock in the morning, and found that all their feverish excitement had entirely subsided; their eyes, necks, and breasts had become tinged with yellow, and Caroline had black vomit. At this juncture it was not easy for one who had marked the symptoms from their commencement with fever to their present malignant change, to be mistaken with respect to the nature of the disease: I therefore in a guarded and private manner, expressed my opinion to a few of my friends, for at this time I did not mean to give the alarm, in anticipation of the measures of the Board of Health, nor even to name the disease to the public.

Monday the 15th—I found Amanda much as on the 14th, but Caroline appeared to be fast approaching to the fatal termination of her malady. After visiting my patients on this morning, I called on Mr. Waldron B. Post, with whom I had some business. He resides in Greenwich near Rector-street, I informed him that the yellow fever had appeared in his neighbourhood, and that the safety of himself and family required his attention to the circumstance; which I believe he immediately did, by looking out without delay for a place of retreat. Towards evening on this day, in a walk down Vesey-street, I met with Doctors Hosack and Francis; I informed them that yellow fever had broken out in Rector-street. Dr. Hosack inquired, whether I had reported it to the Board of Health. I answered in the negative: he replied, "If I knew it to be yellow fever, I would report it as such."

Tuesday the 16th.—On the morning of this day I found Amanda convalescing, and Caroline growing worse; but John, an interesting youth of seventeen years of age, had sickened during the preceding night: from the violence of the first symptoms I judged that his case would in its course become highly malignant. On my way from Rector-street, in Broadway I met Dr. Daniel W. Kissam, in company with a gentleman, with whom I was unacquainted. I informed them that yellow fever had made its appearance in Rector-street. I now proceeded directly to the City Hall, where I reported the cases nearly in the words of the following conversation, which took place between his honour the Mayor and myself:

"Sir, there are three cases of fever under my charge, which I think demand the attention of the Board of Health."

"What are the names of the persons sick, and where are they to be found?"

"They are the children of Mr. Reder, who lives in Rector-street, next door to the corner of Washington-street."

"What kind or description of fever do you think they have?"

"I think it such a fever as our soil and climate are not calculated to produce; a fever depending on the introduction of foreign poison for its origin, and exotic in our country, and such a fever as is likely to do great injury, if it be allowed to spread; and I wish the Resident Physician may be requested to examine the patients and name the disease: and I further wish, that neither Dr. Hicks, nor any of the assistants of the Board, be permitted to visit them at present; if, however, you think it would be agreeable to the Resident Physician, I will wait on him, and introduce him to the patients."

The Mayor replied, "I will write him a note to that effect, and request him to await your call at his own house at four o'clock this afternoon."

Agreeably to the above arrangement, I called, in company with Dr. W. Miner, on the Resident Physician at four o'clock, when the following conversation took place between us:

"Doctor, your patients in Rector-street have bilious fever."

"Have you seen them, Doctor?"

"Yes, after receiving notice from the Mayor this morning, I happened to be in that part of the city, and I thought it more convenient to visit them before dinner."

"Well, I am glad you found the disease nothing worse. I fear, however, it will give you some trouble before you have done with it."

After some other observations on the subject of our difference of opinion, I left the doctor, not at all offended at his having visited my patients at an earlier hour than had been suggested to him by the Mayor.

Wednesday the 17th.—On this day Caroline died, and John grew worse. The Board met at 2 o'clock, when the Resident Physician stated, "that he had no report to make." He had, to be sure, visited my patient in Rector-street, and found them ill with *bilious*, but not yellow fever. Finding the Board about to take no measures on the subject, and that, of course, not only my friends, but also the public in the neighbourhood of the disease, would be exposed to the danger of a spreading pestilence, I resolved at once to throw off the caution, which I had hitherto observed, and state, to every inquirer, my opinion on the whole matter. I accordingly lost no time in declaring to every member of the Board, with whom I was acquainted, and to all its assistants, that the disease in question was positively YELLOW FEVER. Among the members thus informed, were his Honour the Mayor, Aldermen Fairlie and Hall, Doctor Dyckman the Health Commissioner, General Morton the Clerk of the Board, and the Assistants, Doctors Cutter and Hicks, and Captain Mills. Three days after this, Alderman Taylor, of the eighth ward, with whom I was unacquainted at the time, learned my opinion of the disease, in a conversation which he commenced in the Mayor's office, as follows: "Doctor, we think it very strange, that you should tell your friends that there is yellow-fever in Rector-street, when you have not reported it as such to the Board." My reply was, "I mentioned the existence of this fever to the Mayor, on last Tuesday morning, in such terms as, I thought, might call the attention of the Board to it; but, in that report, for particular reasons, I purposely omitted the use of the word *yellow*; but I now tell you, that it is positively the yellow fever, and, knowing it to be such, I consider it my duty to inform my friends of the fact, that they may be enabled to take care of themselves in time. I am aware that the Resident Physician is of a different opinion, but it is immaterial to me, as an individual, what either he, or the Board, may call the disease. We think very highly of Doctor Quackenbos; he was very correct in the year 1819. As a man, I think very highly of him too; but my conduct in this affair must be governed altogether by my own opinion of the nature of the fever in question."

Thursday and Friday the 18th and 19th.—John's symptoms progressed from bad to worse, until Saturday the 20th, when black vomiting began. On the morning of this day the Resident Physician visited him, in company with Doctor Manley, who started a new opinion of the nature of the disease, by calling it "Savannah fever," which, by the by, is a species of bilious fever, that is never communicated from vessels or goods to our shores, so as to become located, and to affect the inhabitants. But, by some means or other, it happened that Doctor Manley was employed to attend John with me, as consulting physician. We saw him together at 12 o'clock, and I observed the Doctor began to think his morning opinion not tenable; and, at his evening visit, he stood perfectly corrected; for, as we left Mr. Reder's house, we met Mr. Noah, the editor of the National Advocate, at the corner of Rector and Greenwich streets, who made some inquiry of us touching the nature of the fever in the neighbourhood; to which Doctor Manley replied in these words: "If I was obliged to give an opinion of this disease, under oath, I should say it is yellow fever;" and to an observation of Mr. Noah, "we are in danger here, if that is the case," I answered, "Yes; while standing on these paving stones, you may consider yourself as knee-deep in pestilence." On Monday the 22d, John died. The only family, besides that of Mr. Reder, living in Rector, between Washington and Greenwich streets was that of Mr. De Lange. This man I advised to leave the neighbourhood, which he did on the 22d, but not early enough to escape suffering, as one of his sons had the disease severely, after retiring to Long-Island. A few days after this time, the Board met with closed doors, in the small room adjoining the Mayor's office. I happened to be in the hall at the time, and was invited into the room before the assembled Board. The Recorder expressed a wish to hear what I had to say on the subject which had called them together. I stated, that "I had been compelled, by the symptoms of the disease in Rector-street, to consider it precisely that variety of yellow fever, which overran this city in the year 1798."

During my attendance on Mr. Reder's children, after much inquiry, whether any other person had sickened in the neighbourhood, I learned that Andrew Thomas, a young Scotsman, had been sent sick, on or about the 11th, from the south-west corner of Washington and Rector-streets, to the New-York

Hospital, where he died on the 16th. I mentioned these circumstances to the Mayor, who caused inquiry to be made, from which it was clearly shown, that Mr. Thomas did die of strongly marked yellow fever in that Institution, without greatly disturbing the quiet of its managers.

As the Board seemed to feel itself bound to consider all information, not derived from the Resident Physician, incorrect or unofficial, it of course continued to act in an undecided and hesitating manner, until the 5th of August, when the alarming progress of the disease forced that officer to acknowledge its true character. Dr. John Neilson, to be sure, after a little disputation with the Board about that unmeaning word *bilious*, did, on the 31st of July, report two cases of this disease as yellow fever, the symptoms of which he well described in a letter to the Board on the following day. His report, however, as it did not come from the right source, like the declations which for two weeks previous I had daily made on the same subject, was insufficient and unavailing towards influencing the decisions of that assembly.

The number of cases continued to increase daily until the 9th of August, when the Resident Physician seemed to think that the public mind demanded some little explanation, in order to enable it to account, in the most reasonable manner, for what was going on. This explanation he made in the following words: "From the above facts it appears evident, that the cause or causes, which at first were only sufficient to produce bilious fever, have now become so concentrated as to create yellow fever." This puzzling and unaccountable concentration of the cause or causes of this disease, if it really took place, was certainly a manœuvre calculated to mislead the most discerning. We have, however, good reason to doubt the fact; as the number of deaths, before the announced concentration of the cause or causes, was fully equal to what happened afterwards, in proportion to the number who really had the disease.

From this time, the Board met daily until late in October, manifesting the greatest solicitude for the public welfare; for all parties acknowledged that yellow fever really existed in town. The table of their chamber now daily teemed with the speculations of the learned, on arresting the progress of the disease, or destroying its cause. These propositions, however, to say the least of them, came six weeks too late; for by this time the infection had spread over a surface by far too wide to be controlled by human power. A general removal, which took place from the lower parts of the city, was the only means of safety left the inhabitants; and the disease continued to spread until the 20th of September, when a very heavy rain occurred, followed by a cold, strong, northerly wind, of two days continuance. The effect produced on the progress of the fever, by this rain and wind, was, in a high degree, striking and sensible. The out-of-door poison, or that part of it which had possession of the streets, seemed to be entirely destroyed; the extension of the infected district, which until this time had been gradual and well marked, ceased; and, in four or five days, the cases diminished, until the eighth after the change of weather, when not one was reported to the Board. Shortly afterwards, however, the poison again showed itself; but the subsequent cases appear to have arisen from infection remaining in the houses, and not to have been acquired in the streets, as was frequently the case before the storm.

Having given, above, as much of the late fever, in connexion with the relative proceedings of the Board of Health, as I think due to the public interest, the public curiosity, or to my own reputation, I shall proceed to the consideration of the more important parts of my subject: and the first and most important question to be answered, is, Where did the poison which caused the fever of this year, come from? This question has been, and will continue to be, answered by the advocates of different theories, in such a way as to give the most plausible support to their favourite opinion. The exclusive believers in home or local origin, will ever find an old sink, a cess-pool, a rotten potato, or a putrid mouse, or some other nuisance of the kind, sufficient, if one could believe their stories, to poison a world. These gentlemen, however, do not seem to recollect, that the nuisances, from which they fear so much, are the hourly productions of nature, and abound every where, at all times, over all the earth, rarely causing disease among its inhabitants. I am willing to admit, that under peculiar circumstances of confinement, or want of ventilation, accumulations of filth may excite typhus fever of a very malignant type: I, however, do not admit, but assert the contrary, that the exhalations from any or all these nuisances have the least influence on yellow fever, either to render it more or less malignant, or to retard or hasten its progress; for yellow fever is for ever precisely the same disease, with its power to kill unimpaired, whether it appears in the healthy atmosphere of Middletown, in Connecticut; the Wallabout, Brooklyn; the Quarantine Ground, or the more impure atmosphere of this and other large cities; and all the stories, even admitted as true, related of churches, cemeteries, sinks, jails, and processes of putrefaction, are not worth a jot on the present question: they prove nothing; as the relaters of them have to show that they ever, in a single instance, have caused yellow fever. On the question of cleanliness, they are, however, of inestimable value, as they prove the absolute necessity of rigidly enforcing the observance of that virtue. Dirty streets, yards, and damp, filthy, unventilated houses, not only originate several varieties of fever, but sensibly aggravate most diseases to which the human frame is liable; and when these accumulations of filth happen to be assisted by the most vicious habits which idleness and drunkenness can engender, they frequently cause such a fever as appeared in Bancker-street two years ago, which killed negroes by the dozen, and desperately pothered some gentlemen of the faculty, setting them agog for endemic yellow fever.

Church, jail, and Bancker-street fevers are always caused by a poison, generated and existing within the apartments in which the sick are found; and to remove the cause and restore salubrity, it is only necessary to cleanse and ventilate. But with yellow fever the fact is otherwise; this is always caused by an out of door poison, which first gains possession of the streets, from which it enters with equal facility the palaces of the rich and the hovels of the most wretched; carrying to both the same power to destroy. Ventilating and cleansing are here of no avail, until the stormy winds and low temperature of approaching winter have driven the enemy from the streets.

New York has the reputation of being a dirty city, or at least, it has to my knowledge been constantly complained of as such for the last twenty-five years; some parts of it are, however, infinitely more dirty than others. The Collect, and Swamp, for instance, may be said to be natural cess-pools of very great extent, each of them containing many acres of made ground, and each of them receiving, from time immemorial, as in a place of deposit, the wash and filth of a large surrounding surface. Yet these places, although ten times as filthy as any other part of the city, have always been remarkably exempt from yellow fever. This fact is, however, easily accounted for, in the uniform manner of attack pursued by the disease in question, which is invariably on the wharves, by the side of vessels, and no where else: hence, it evidently appears, that if our city filth has any agency in producing yellow fever, it is only the filth on the wharves; for all the experience we have on the subject goes to prove, that this disease never made its first appearance in the interior of this or any other northern city.

Before meddling with that fruitful bone of contention among the different theorists who speculate on this subject, to wit, the consideration of the causes in and about Rector-street, whether of foreign or domestic origin, which could have contributed to the production of the late fever, I will proceed to make a few observations on what I deem an incorrect and unphilosophical habit indulged by too many, in forming their judgment of things altogether from external appearances, or on the sensible qualities of taste and smell. This habit appears to be partly natural and partly acquired, but regulated in some cases by our experience. Toads and spiders are charged by thousands with a high degree of venom, merely because their Creator, in his wisdom, did not think proper to give them an aspect corresponding with our notions of beauty:

and even with our ample experience of the perfectly harmless natures of these little animals, we cannot look at them without feelings of hostility : and a man ignorant of the effects of aloes and arsenic, would, on account of its offensive taste, throw the former from his mouth, as poisonous ; while, without suspicion, from its insipidity, he would readily swallow in a dish of tea enough of the latter to destroy him. In no way, however, has the mind been warped by prejudice from correct and just conclusions, so strikingly and so frequently, as where offensive impressions have been made on the organ of smelling. This has been more particularly the case in this city for the last twenty-five years. Prior to the commencement of that period, the people, like the inhabitants of the country now, could approach the offensive atmosphere of a putrid carcass, a mud hole, or a heap of manure, without ever once thinking of yellow fever. The frequent introduction, however, of the invisible poison, which causes that disease, in a way unaccountable to such as would not be at the trouble of acquiring correct information, has led thousands, and among that number some physicians, to ascribe this fatal malady to the nearest offensive object of the nuisance kind, that happened to exist at the moment on a neighbour's premises. For, curious as it may appear, I have never yet been able to find a man who would acknowledge any nuisance about his own house, however offensive, capable of producing disease ; but when his family sickened, it was caused by spoiled provision in his neighbour's cellar, or by a mud-hole, or a broken sink on an adjoining lot. These people never once think of correcting their opinions by past experience. If they did, by referring back to the half century preceding the year 1798, they would learn that any or all these nuisances were never so mischievous in those days as to cause yellow fever. Previous to the year above-mentioned too, no regulations of the least use had been adopted for keeping the city clean, and nothing was more common than to see dead cats, dogs, and even hogs, broken up by the cart-wheels in every part of the streets. During a visit which I made a few days ago to the neighbourhood of Rector-street, I could not but observe the application of the foregoing reasoning to the particular subject under consideration. The inhabitants of that vicinity appeared to be divided into four classes, three of which were altogether *led by the nose*, to make answer to this question, which I put to a number of highly respectable gentlemen: "What do you think caused the late fever among you?" First class, "We think it must have been caused by the cess-pool, for it emitted a very bad smell." Second class, "We think it must have been produced by some puddles of water and filth, which stagnated in the gutters, and became, from the heat of the weather, very offensive to the smell." Third class. "We have no doubt but that it originated from the contents of the sinks, which were thrown into the street about that time, for the stench of them was intolerable." And a fourth class, who had not been seized so unceremoniously by the nose, believed it to be derived from vessels or goods brought to the wharves.

So great is the disposition of many, to ascribe mischievous powers in producing disease to all substances of strong and offensive scent, that I should not be surprised to find some hereafter ranking assafœtida, hepar sulphuris, and other medicinal drugs, among the causes of yellow fever. It is, however, certain, that yellow fever has no necessary connexion with sensible smells; for allowing the poison, which causes that disease, to arise from nuisances of any kind, in a state of mixture with the materials of stench, the combination exists only for a very short time, as after every thing sensible to the nose has been driven off, and dispersed by the wind, the infection will continue for months to spread in every direction, as inodorous as it is invisible.

The sources of mischief, in or near Rector-street, which have been spoken of by the different parties to this question, as having a supposed agency in causing the late fever, I shall divide into two kinds: first, those on shore, of local, or home production; second, those at the wharves, derived from goods or vessels, and of foreign origin. Of the former kind there are three; and, at the head of these, stands the cess-pool.

As this is a word only very lately brought into use, and in bad company too, it is necessary to give a very particular description of the thing signified by the name, in order that no reader may attach to it some magical meaning of horrid import. A cess-pool is, then, nothing more nor less than a little, paltry, dirty, artificial mud-hole. The cess-pool, now to be particularly described, was situated two or three feet from the south end of an alley, which runs north from Rector-street, on the east-side of Mr. Reder's ground, and across the rear of three lots which face Greenwich-street. It was placed there four years ago, when Rector and Greenwich-streets were raised above the adjoining land, to receive the wash and filth of the four lots bounding the alley in which it is contained. It was three feet deep, and four feet in diameter; planked up around the sides, and covered with an iron grate; and, like all other mud holes, it was sometimes, during the heavy rains, filled to overflowing. Five months previous to the beginning of the fever, it was cleaned out; and. about the beginning of July, the water which it contained was taken from it, and thrown into the street. Of the stench of this nuisance I have spoken above; some of the inhabitants complained of it; others did not. Some time, during the last week of July, it was particularly examined, by order of the Board of Health, by Doctor Hicks, and two other persons, and found, though not very offensive, to contain a quantity of mud which had not been disturbed, when the water was removed a month before. This mud was taken out by two men, put into a cart by a third, and the hole filled up by a fourth. Of the seven persons employed about this affair, not one of them contracted the fever.

Before touching particularly upon the second source of offensive matter, which some persons have spoken of as having a supposed agency in producing disease, viz. several new and old sinks near Rector-street, it is necessary to say something of the nature of the ground in which they are located. All this shore of the city, west of Greenwich-street, is what is commonly denominated "made ground;" that is, it has been raised above the flowing of tide-water, by earth, or some other material, brought from a distance. The lot which runs from Greenwich-street to the water's edge, south of Rector-street, on which all the jakes in question were located, was formerly occupied for many years, as a stone-cutter's yard and shop, and was altogether filled up with the chippings of marble and sandstone, in pieces of every dimension, from several pounds weight, down to the smallest grain of sand. Earth, as I shall call it, for want of a more appropriate name, thus made, is hard and very porous, and freely admits the flowing tide-water through it in every direction.

The first pit made for a new sink, was directly in the rear of, and a few feet from, Mr. Bakewell's duck store. It was at first contemplated to make this pit ten feet deep; but, from the inconvenience experienced and expected, from the free ingress of the tide, it was carried to no greater depth than seven feet. The earth taken from it, of the nature above described, had no offensive smell or appearance, and was thrown into Washington-street. where some hollows required filling up. The next pit made for a new sink, was thirty feet east of the one above described, in the same kind of earth, and the same difficulties were experienced from the ingress of the tide; but the circumstance which gave the labourers the most trouble, and the sink a bad name, as causing fevers, was in consequence of the spot chosen for the pit, being so near to one which was in daily use, on an adjoining lot, that when the workmen had descended to the depth of five feet, a part of the thin partition of earth between the old sink and the new pit gave way; thus permitting a small portion of the contents of the former to escape into the latter. This was necessarily removed with the earth, to the annoyance of some individuals in the neighbourhood. It was not, however, suffered, as some have stated, to lie in the street, but was carted to the battery, to fill up a dock in that vicinity. The number of persons employed, from the middle to the last of July, about this affair, were from twelve to twenty, not one of whom was attacked with fever.

The third nuisance, mentioned by some as a cause of disease at this place, was the stagnant water which remained in the gutters until it became offensive. Rector-street, however, from its descent, will not permit water to stand in any part of it in greater depth than two inches.

The above enumeration includes every thing which has been spoken of as a cause of the fever on the spot where it first appeared. Some ingenious persons did, however, labour hard to prove that the source of the mischief was to be found in the burying ground attached to Trinity church. In order to confute every argument which they have advanced in support of their opinion, it is only necessary to state the fact. that the poison which actually did cause the disease, was spreading daily in every direction, from the place of its first appearance, for one month before it reached the vicinity of that churchyard.

The second grand division of causes which were likely to originate the late fever in Rector-street, comes next to be examined. These, all of foreign origin, are to be sought after in suspicious vessels, which were permitted to come to the wharves in that vicinity, or in goods landed there from vessels known to be infected. The officers of the quarantine establishment for this city, have designated, as four-day-vessels, a certain class, which are always to be regarded as suspicious, because, owing to a defect in the law, they are privileged to carry pestilence wherever they go; thus jeopardizing the lives of thousands, and rendering all other quarantine regulations useless. To prove the above allegation, it is only necessary to state a few facts. First, it is well known, that the different Boards of Health of the cities and towns on our seaboard, are, five times out of six, among the last public bodies, which come to the knowledge of pestilence in their respective districts. Hence, in almost every case where a pestilential fever appears in any city, from one to two months elapse before its Board of Health become officially informed of it. During this period the public authorities of the place continue to issue clean bills of health to every vessel sailing from it. One of these vessels, though infected with the poison of yellow fever, may arrive at this port with a healthy crew, or one or two of her hands may have sickened or died on her passage, and the officers, as it is always their interest so to do, may disguise or conceal the real character of the disease. This vessel, on her arrival here, is liable to be stopped only four days at the quarantine ground, to undergo the too often useless operation of white-washing. She is then permitted, by law, to come to our wharves, with a strong probability in favour of her kindling pestilence wherever she touches. Cases of the above description are by no means of rare occurrence; it is, however, unnecessary to mention more than one or two. The ship Asia sailed from New-Orleans some time in the month of August last, and arrived here on the 18th of September. She left three of her crew sick with yellow fever, in the hospital of that city; yet, strange as it may appear, she brought a clean bill of health, signed by the proper authorities of the place. And, during the first three weeks of the late fever in this city, it is well known that vessels sailed daily from this port with clean bills of health: one of these, the ship Illinois, however, which left Rector-street wharf, on the 16th of July, did lose a passenger by yellow fever.

Again, a vessel may cruise between the different ports in the West Indies for months, and at last venture near enough to some sickly place to receive the poison of pestilence on board, which may, or may not, affect her crew, on her passage to the north. She may touch at Charleston, or some where else in the southern states. When she arrives here. she reports herself healthy from the last place she was at, within the limits of the United States; she is then, by law, detained four days at the quarantine ground, for purification, from whence she is permitted to come to the city to discharge her cargo, with a possibility of poisoning whoever approaches her.

To the four-day class belong all the suspicious vessels, lying near Rector-street wharf, at the commencement of the late fever. These were five or six in number; first, the schooner Florida, Captain Johnson, of which much has been said. This schooner was employed as a cartel, in 1821, to transport to the Havana, at the expense of the Spanish government, such citizens of St. Augustine, as chose to leave that place upon its cession to the United States. She arrived at Havana in the month of August, crowded with passengers, at the time when that place was extremely sickly with yellow fever, which was soon introduced on board, affecting a number of persons, several of whom died, and her commander had the disease. In September, she returned to St. Augustine, where it is stated, that Captain Johnson sent a quantity of clothing on shore, which was washed by a woman, who had a husband and three children, all of whom, five in number, were poisoned by the clothes, and all died with yellow fever; the first victims of that disease on shore for a number of years. The poison, now located at the house of the washerwoman, spread from thence in every direction over the town, attended by its usual accompaniments, death and desolation. The Florida, some time after this, sailed for this port, and arrived here in November: she sailed again for Cape Florida on the 26th of the same month.

Here a question arises, whether a few days exposure to our November temperature was sufficient to destroy the infection, of which no doubt she contained much? She spent the last winter in the West India seas, part of the time near Cape Florida, but occasionally looking into the Havana: in the spring she became very leaky, and about the first of June sailed for this port for the purpose of repairing: after stopping at the quarantine ground four days, she arrived at a wharf near Rector-street: on the 21st of June her little cargo, consisting of old iron, old junk, snuff in bottles, &c. was taken out and placed on the wharf, and two large turtles which she brought, were made fast with ropes, and put into the water alongside of her. She was then hove down, her bottom taken out and a new one put in. In her repairs, from six to twelve men were employed for three weeks, none of whom were taken ill, except her blacksmith and his apprentice, who, according to their own statement, were both on board her, and both among the first affected with the fever. Besides this blacksmith and his apprentice, five others, four of whom are since dead, are known to have visited this vessel, or the dock at which she lay, viz. John Reder, Andrew Thomas, Leonard Archer, William Kimball, and William Morgan. Now I have not told this long story about the schooner Florida to prove, or even to infer, that she was an infected vessel when she arrived, but on the contrary, I think with our able health officer, that she was not. She however, unintentionally I think, did some mischief; for by placing her turtles, one of which was so large as to attract much notice, alongside, she drew many, especially children, who were inclined to see those animals, into the very centre of yellow fever poison, which was then discharging from another source to be noticed hereafter.

The next vessel to be mentioned, is the ship Shamrock, from Matanzas, which arrived on the 11th of June: she was quarantined four days, and then permitted to come to the wharf near Rector-street, although it was soon discovered that she had sailed from a sickly port. We have however reason to believe she fortunately escaped infection; so with the Elias Berger and schooner Nile; they were both from sickly ports, though believed to be otherwise at the time of their arrival, and both quarantined four days. They did not, however, either of them, arrive early enough in the season, whether infected or not, to have caused the yellow fever. The last vessels I have named, with their attending circumstances, principally to show the absolute necessity of putting the four-day vessels under very different regulations, if the health of the city is to be preserved by quarantine establishments.

The part of my subject next to be considered demands the most serious attention from every inhabitant of this city, whose wishes and exertions are united for the attainment of that invaluable blessing, the public health; as it shows, how absolutely the appearance of that particular kind of pestilence, called yellow fever, is enchained to an intercourse with known and admitted foreign infection. Indeed, it may be truly affirmed, that the substance does not more certainly regulate the movements of the shadow, than does the intercourse here mentioned, the appearance of that fever in this city and its neighbourhood; and here I beg leave to state a curious fact, but whether one to be laughed at, or treated more gravely, I will not now decide, viz. that the sticklers for the local origin of this disease have never yet found a nuisance which was, in their opinion, sufficient to produce it, but it happened some how or other, to be always located in the very centre of this intercourse with foreign infection. We find, that during the last twenty years, without going further south than Perth Amboy, the yellow fever has been communicated to this city, or to some town or place in its vicinity, in more than twenty instances; and in only two or three instances out of these more than twenty, has there been the least difficulty experienced in pointing with certainty to the vessel which brought it, or to the intercourse through which it was communicated; and in the two or three instances, where the poison has not been fairly traced to the vessel which brought it, it has been owing to the want of proper inquiry timely made on the subject, or to difficulties inseparable from such inquiries: for the persons who know most about the particular facts, are frequently much disposed, for obvious reasons, to hide or at least discolour them. And where were these two or three instances of pestilence found, in which some difficulty was experienced in tracing them to their proper sources of infection? Not at the Wallabout, nor at Brooklyn, nor at Perth Amboy, nor at the Quarantine Ground, nor at Middletown, nor New Haven, nor New London. No, it was at none of these small towns, whose most minute intercourse is continually exposed to the public eye; some of which have, however, suffered severely several times by the introduction of yellow fever. It was in New-York that the difficulty was found, a city, including sojourners, containing a population of 140,000 souls, and probably enjoying a larger commerce with sickly places than any other in the universe of the same number of inhabitants. A city, not like many in Europe, surrounded by a "Corps Sanitaire," armed with bayonets to prevent the introduction of foreign pestilence, but depending for protection in that respect on a quarantine establishment, which is regulated by laws so unaccountably formed, as not only to admit daily to its wharves, vessels as likely to be infected as otherwise, but also to admit to any part of it, during the greatest heat of summer, a cargo known to be infected, or even three or four such cargoes at a time. And, as it is now admitted, because proved beyond the power of contradiction, that a sailor's bed, or even his bundle of clothes, may convey enough of the subtle and invisible poison which causes yellow fever, from a sickly place in a distant country to a healthy family or community in this, to originate that disease with all its horrors; it is to me really a matter of astonishment, that hundreds of instances have not occurred, which could not be traced at all, instead of two or three, which have presented difficulties on that account.

With the foregoing general observations touching the subject under consideration, I have thought it best to preface the particular instance of intercourse with admitted infection, now to be related. It seems our merchants claim under the colour of law, or long usage, the privilege to bring to this city in lighters the cargoes of all vessels at the quarantine ground, which may have arrived there from sickly ports, and consequently subject to a detention of thirty days, regardless of any disease or mortality that may have occurred on board them. Of this description were the four following vessels: viz. first, the Spanish Soldier, which had sixteen days passage from the Havana, and arrived on the 25th of June: one man was sent on shore sick of yellow fever before she sailed, who died on the 30th of May; another sickened of the same disease, and died on the 17th of June during her passage to this port. Second and third, the brigs Ambuscade and Abeona; the former of these vessels had thirteen days passage, the latter fourteen; they were navigated by acclimated crews, composed chiefly of Spaniards, who of course were not subject to yellow fever: on their arrival here they reported the Havana as very sickly. Fourth, the ship Eliza Jane; she had thirteen days passage, and arrived here on the 1st of July: two of her crew died of yellow fever, one on the 10th of June, just before she sailed.

the other on the 18th of the same month, while on her passage. The cargoes of these four vessels, which I have named above and described, as known and admitted by all parties to be infected, for the reasons above mentioned, began to come to the city on or about the 28th of June; and between that day and the 6th of July, nearly two thousand boxes of Havana sugar had been landed within a space of two hundred and forty yards, of which space the bottom of Rector-street appears to be the centre. On the 10th of July, five days after the principal part of the infected goods, viz. twenty four lighter loads had been landed, the first cases of the fever appeared. The merchandise, to which we charge the introduction of poison in this instance, was wood, made into boxes containing sugar, and therefore judged by many to be incapable of imbibing infection. This opinion, in connexion with some others, which seem to have influenced the legislature at the time of their passing the laws establishing quarantine regulations, I wish to examine, because I believe them to be not only very erroneous in themselves, but very dangerous to the interests of this city, when made the basis of practice.

The opinion above alluded to, held forth by the law and by the merchants on this subject, appears to be founded on the belief, that there has been conferred on man, through the operation of his organs of sense, a capability of distinguishing so nicely in this most difficult and dangerous of all cases, as to enable him to point out with certainty the several articles of an infected cargo, which have imbibed the least or the greatest quantity of the poison which causes pestilence. Now, as it is well known that no such power of judging aright has been gratuitously given to man, and that he only becomes informed of the existence of the cause from its dreadful effects; and as the executors of the law, who in conjunction with others of the same opinion, under the influence of its erroneous doctrines, have made a few experiments to which, though originating from the best intentions, thousands of their fellow-creatures have fallen victims; and as it is certain that the members of the legislature, in common with the rest of mankind, have been at all times ignorant of the nature, composition, and chemical affinities of the poison of pestilence; and as it is moreover equally certain that the few facts, which have been slowly developed and arranged mostly in this country, during the last twenty-five years, and which serve to throw a faint gleam of light on some of the laws which seem to govern the causes of pestilence, have not been called in to the aid of those whose duty it is to guard us against its approach, either in the correction of their opinions, or the improvement of their practice; is it to be expected, I ask, under all these circumstances, that the result should have been more favourable than it really has been? or that the best laws should not have been devised and passed for the protection of the public health; and that the practice under these laws should not have been the safest which could have been adopted? All, or indeed the greatest good that could have been expected from legislative bodies, surrounded as they always have been with darkness and difficulties, would have been the passing of a law enforcing yearly the total prohibition of intercourse between this city and infected vessels and their cargoes from May until November: and whenever our experience, founded on facts, can convince us that particular articles of merchandise may be safely exempted from quarantine vexations, then, and not until then, will it be time for enlightened legislators to step in and pass laws in favour of the exemption of particular articles.

The few facts hinted at above, from which I have thought some conclusions of practical utility might be drawn, I beg leave to state as follows, viz.

First.—Great heat, with drought so long continued as nearly to suspend the watery exhalations from the earth, check, in the same proportion, the progress of yellow fever.

Second.—The greater safety in visiting an infected spot in the time of night, after the aqueous vapours have fallen to the earth, than in the time of day when they are rising from it.

Third.—The difficulty experienced in all attempts to cleanse an infected vessel, either by washing, white-washing, ventilation, or fumigation.

Fourth.—The tenacity with which infection adheres to any spot of earth where it once becomes located.

Fifth.—Very heavy or long continued rains, by thoroughly washing the earth's surface, appear to dispose of or carry away so much of the poison of pestilence, as sometimes to almost suspend its progress. This effect was observed at the Old Slip in 1819, from a very copious fall of rain which occurred in October, and also during the late fever from a like storm, which happened on the 20th of September.

Sixth.—The moment any considerable difference of temperature takes place between the earth's surface and the stratum of air in immediate contact with it, though freezing does not follow, that is, when the air is cooled fifteen or twenty degrees below the temperature of the earth, the cause of yellow fever becomes very much weakened, and sometimes altogether checked.

Seventh.—Whenever the temperature of the atmosphere is so low that water is changed to ice, pestilence is invariably stayed.

The above facts have been so uniformly developed in the economy of that kind of pestilence, known by the name of yellow fever, that they may be considered as well authenticated, and from them the following conclusions appear to me to be naturally deducible:

First.—That the poison which causes this disease, be it what it may, unites very readily with water, and in that state of union penetrates freely every substance which absorbs that fluid.

Second.—That this poison cannot act noxiously on the human frame, but through the medium or agency of water; nor even through that medium or agency unless that fluid shall have passed into the state of vapour.

Third.—When aqueous vapours cease to arise from an infected spot of earth, or infected goods, in consequence of extreme dryness, or any other cause, the only operation by which the poison of this pestilence can be applied to man also ceases, and disease is invariably stayed, or, at least, the danger of contracting it greatly diminished, according to our first and second facts.

Fourth.—All articles capable of absorbing water, and which have taken up that fluid in union with infection, are to be regarded as dangerous to the public health, in proportion to the quantity imbibed; and the time of this danger will be more or less protracted, as they dry more or less slowly. And the wood of which ships are composed, and that found among the cargoes of infected vessels, become highly imbued with this poison, for this article absorbs water like a sponge; and in consequence of drying more slowly, it continues to throw out infection for a longer time than most other materials. This is the reason why a ship may be so cleansed by washing and ventilation to-day, as to be visited with comparative safety; but in the act of drying next week, she will throw out so much poison as to become re-infected herself, and to infect the atmosphere in her immediate neighbourhood; and thus, also, the water which has become infected, and has sunk two or three inches into the ground will remain there, undisturbed by the wind, until the action of a warm sun shall bring it forth in a state of vapour, to do the work of mischief and death, according to our third and fourth facts.

If the conclusions here are fairly drawn and warranted by the facts, the sugar boxes, which were landed by the thousand at the bottom of Rector-street, in the beginning of last July, were unquestionably the most mischievous articles which could have been brought into this city. If very large quantities of pure wholesome water be suddenly thrown upon infected spots of the earth, the poison of fever is carried to so great a depth below the surface, or so washed away, as to be rendered nearly harmless, according to our fifth fact. Here I beg leave to suggest the propriety of introducing into this city a supply of good water, sufficiently large to wash freely and frequently over any part of it that may become unhealthy.

During autumn, as the influence of the sun becomes diminished, and the cold northerly winds begin to prevail, the atmosphere is cooled much more rapidly than the surface of the earth; hence the watery exhalations from it will be immediately condensed, and returned to it without their having obtained sufficient elevation to become very mischievous, according to our sixth fact. The condition of water is so completely changed by being frozen, that all evaporation from it of the least importance ceases; hence it is no longer the medium for the circulation of the poison of pestilence, and the poison of pestilence also ceases to circulate for want of such a medium, and the watery vapours afloat in the atmosphere, cease to be united with pestilential miasm, in consequence of their precipitation to the earth in the form of hoar frost; thus purifying the air, and staying pestilence, according to our seventh fact.

If what I have stated for facts, are really such, and I have no doubt they are, and if the conclusions which I have drawn from thence are just, as I believe them to be, the first step may then be said to be made towards the establishment of something like rational quarantine regulations; which I do not think exist, at present, in any part of the commercial world: for while our own are so loose as to permit infected vessels and cargoes to pass them by wholesale, those of Europe are so rigid as to quarantine a vessel for forty days in one of their out-harbours, although she may have arrived in the middle of winter, so overloaded with ice, as to be in danger of sinking from it. For this purpose it is only necessary to make these practical distinctions between the several articles of merchandise which may come into our ports, on board of infected vessels, and to divide them into three classes: First, such goods as do not imbibe water at all: to this class belong all metals, and articles formed of glass, marble, &c. These, to be cleansed, need only to be well washed and thoroughly dried. No washing and drying, however, which have been done on board of infected vessels, or in infected districts of a city, are to be relied on with perfect safety. Second, such goods as absorb water freely, and part with it quickly: to this division belong clothing and bedding, and, if not too closely packed, even cotton and sponge; repeated washing and dying will, no doubt, cleanse these articles. Third, such goods as take up water freely, and part with it very slowly; for instance coffee and hides, if large quantities of them be together. At the head, however, of articles of this class, always charged for the work of mischief, will be found infected vessels themselves, so long as they continue to be made of wood, and the portions of their cargoes, which are composed of that material. In cleansing these portions, the exercise of that great virtue, long patience, seems to be absolutely necessary, as the poison they have imbibed lies out of the reach of ordinary washing, fumigation, or ventilation. Certain agents may be found, from their avidity for water, possessing great exsiccant power; among which are quicklime, and some others. These may, therefore, be sometimes used with advantage to intercept the matter of poison, or to render it inert, by depriving it of its medium for circulation; but they ought always to be employed in a state of extreme dryness, and not saturated with water, as in white washing. Indeed the lime, which was spread over a part of the infected district, during the late fever, might have done some good, if those who placed it there, had not taken effectual measures to render it *useless by slaking it at the time of laying it on.* And as we know that any thing placed alongside an infected vessel, is liable to become itself infected; and as we also do not know how small a quantity of this infection, when introduced into our city, may originate a most desolating pestilence, I cannot avoid thinking that it would be our wisest course to run no risks on this head. I therefore beg leave to suggest the propriety of preventing those vessels called "lighters," from approaching the wharves for at least some days, after having lain for hours in contact with vessels known to be poisoned, whether they have received goods from them or not: neither would I permit goods of any description to be brought into the city in the same lighters which conveyed them from the vessels to the place of their purification.

The detention of persons who may arrive at the quarantine-ground, on board infected vessels, may be with perfect safety reduced under plain and easily practicable regulations to one fourth of the time now prescribed by law. The adoption of a reasonable practice in this particular, would go far towards removing quarantine vexations.

The following speculations and opinions, interspersed with facts, are thrown out merely to invite the attention of the curious to this subject.—By the time the poison, which causes yellow fever has passed fifty yards, more or less, from the source whence it is emitted, it has become too much weakened by diffusion to excite that disease, and no length of time, the source continuing of the same extent can enable it to act hurtfully to a greater distance; because in an ever-changing atmosphere, it is constantly and rapidly passing to the point of diffusion. Fifty yards I will suppose to be the average distance to which it can act with effect in this country, from its source of emission, varied however a little by different states of the atmosphere. In some tropical climates, however, this poison appears to be carried with noxious powers to a much greater distance, and the following seem to be the reasons why this happens. In some of these countries in which the poison is produced in great quantities, or found to abound, a profuse watery exhalation arises from the surface, elevating with it into the atmosphere much of this poison, which, owing to the considerable difference of temperature, between the day and the night, a large quantity of this vapour is rapidly condensed, and returned towards the earth in the form of a West-Indian dew, which has been generally remarked to produce the yellow fever. The moment this condensed vapour begins to descend, what is called the land-breeze sets in, which always blows from the land to the water, no doubt carrying with it much of this infected dew; hence, the danger of vessels approaching within a mile or two of the shores of these countries, if sickly, especially during the night.

But yellow fever does extend its ravages, gradually and slowly over large cities; its source then by some means or other, must become proportionally extended: now it is evident, for the reasons given above, that this extension cannot be effected in the atmosphere above the earth's surface; we must therefore look for some place where it can be effected, and this place we find at the surface of the earth. The humidity in that situation seizes the poison with the greatest avidity, either to be carried into the atmosphere by evaporation from the production of disease, or to be washed away out of mischievous circulation, if the spot, where it becomes located, should be inundated with water. If, at the time of the location of this poison on a spot of earth, heat and moisture are properly combined, so as to favour its operation, the extension of its district begins, I apprehend, in the following way: a portion of the poisoned vapour, which arises from an inch of infected surface, falls on the adjoining inch of uninfected surface, and by it enables that, in its turn, to poison another, and so on; thus slowly enlarging the infected part of a city, until pestilence spreads far and wide.

Of the chemical nature, or composition of the poison of which we have been treating, nothing appears to be known. We have, however, reasonable grounds for believing that it is not of animal origin, and therefore does not excite a disease personally contagious. It is not of animal origin; for, in countries where it most abounds, all the inhabitants being acclimated, the fewest persons have it: it is there the stranger's disease; and all the facts, experiments I will call them, which have been quoted to prove yellow fever personally contagious, have been too loosely made. They do touch this question, but they prove something else, which is, that this fever is personally infectious; that is, the poison which excites it may be carried in any articles of clothing on or about a first person, so as to communicate disease to a second, whether the first may have the disease or not. This is personal infection, as I understand these words.

I said above, that this infection did not act hurtfully more than fifty yards from its source of emission; but after it has passed these fifty yards, and become too much diffused to act noxiously, does it cease to act at all? I think not; for I believe it to be a stimulant of the highest exciting powers when applied to the human body, and that, like all other poisons, when the dose becomes too small to kill, it cures. Suppose we administer to a weak man too large a dose of opium or arsenic, what is the consequence? Why, he is stimulated into painful sensations, disease, and death: but if we administer to the same weak man, small doses of either of these active poisons, he is stimulated into pleasurable sensations, strength, and health.

From this view of the poison which excites yellow fever, I am led to account for a fact, which occurs so universally as to be generally noticed, which is, that when that disease is spreading, for a considerable distance in advance, of the infected district, all other diseases, especially those of debility, seem to be banished or suspended. This suspension of such diseases, I think may be ascribed to the medicinal effects of this stimulant poison, which is diffused through the atmosphere in quantities too small to cause disease.

Yellow Fever in Cheapside and Lombardy Streets.

When one half of a large city is so overspread with pestilence, that thousands of its inhabitants have been forced to fly to its more healthy parts for safety; and when the intercourse of necessity, of business, of friendship, of curiosity, and of knavery have each had their share of influence; it appears very idle to expect to trace every case which may occur elsewhere, to the district first infected: each kind of intercourse here mentioned, occasionally, no doubt, has its victims. When, however, the communication has been carried on for the purposes of knavery, it is not very likely that those who have sickened from it, will be very communicative on the subject, unless the facts be wrung from them at the hour of death by the pangs of a guilty conscience. The particular cases of fever now to be noticed, were caused by the intercourse of friendship or affection: and, as we should expect in all like cases, we find in this, that the family who suffered most, held more communication with admitted infection, or with the infected district, than all the people of the Seventh Ward beside. The facts are these: on or about the 7th of August, Mr. Samuel Ward, who resided in Lumber-street, within the infected part of the city, sickened with yellow fever, and was reported as such, by Doctor Perkins, to the Board of Health. Here he lay ten days very ill. During this time, his father's family, who lived at No. 20 Cheapside-street, manifested on this occasion the most laudable anxiety for his welfare. He was not only visited daily, or oftener, by his father and mother, but his brother, a dumb boy, fifteen years of age, and a sister of thirteen, were constantly with him, as nurses or assistants. They slept at his house every night until the 17th, when it was thought that he had so far recovered as to enable him safely to leave Lumber-street, which he did, and went to his father's house, from whence he set out for the country next day. He brought with him from Lumber-street a quantity of clothing, which, however, was said to have been washed there; that is, washed in pestilence, and dried in the poison of pestilence. About five days after he was gone, his brother and sister above-mentioned, who had returned with him to their father's house, both sickened with fever. The sister's case was not very severe; but the brother was extremely ill, and as he could not speak, he expressed his sense of pain by laying his hand on his head, his back, and stomach. On the fourth or fifth day of his disease, he was taken with profuse bleeding from the nose: after this, they both gradually recovered. Now, there is not in my mind any manner of doubt, but that both these children had yellow fever. A few days after this, another daughter of the elder Mr. Ward was taken ill. Doctor Boyd was called in, who, no doubt very correctly, reported it yellow fever: and in a few days more, the elder Mr. Ward and another son were seized with the same fever, both of whom died. Mr. Ward's house, and its immediate vicinity, may now be fairly considered a second infected district, located only about sixty or seventy feet from No. 4 Lombardy-street; where, it appears from the following affidavit and certificates, that another spark of the poison was set afloat:

No. I. (COPY.)

Jonathan Darrow, cartman, Number 5 Jefferson-street, in the city of New-York, being duly sworn, deposeth and saith:

That on or about the second day of September, in the present year, he was requested by a young man, at or near the New-market, at the bottom of Catharine-street, in the city aforesaid, to take up and put on his cart, two chests, a bed, a hammock, and a bundle of clothes, tied up in a blanket. This young man had with him a sailor, as from his dress he appeared to be, who appeared to own a part of the things aforesaid. The young man first mentioned, after the articles were put on the cart, got on himself with them, he then requested this deponent to drive to number four Lombardy-street, which request was complied with. While on the way thither, a conversation took place between this deponent and the said young man, in which the latter observed, "a part of these things now on the cart does not belong to me, but they did belong to a young man who lately died of yellow fever," or words to that effect. And this deponent further says, that he drove his cart to the house, number four in Lombardy-street, where he delivered that part of the goods, to wit, one chest, one bed, and the blanket containing the clothes, which the young man above mentioned had charge of, to an old woman and a young woman, who both received the goods, and placed them in the entry of the house number four Lombardy-street. And this deponent further saith, that he then drove into Bancker-street, about half-way between Catharine and Market streets, where he delivered the remaining part of the goods, to wit, one chest and a hammock And further this deponent saith not.

(Signed) **JONATHAN DARROW.**

Sworn before me, this 18th day of December, 1822.

(Signed) **HARRIS SCOVELL,**
Assistant Justice.

No. II. (COPY.)

We, whose names are hereunto subscribed, hereby certify, that we saw, during the first week of September, in the present year, several different times, a chest on which was placed a bundle of clothes or bedding, as they appeared to be, tied up in a blanket, as we believe, standing in the entry of the house number four in Lombardy-street.

Given under our hands, the 18th day of December, 1822.

(Signed) **WILLIAM H. MOTT,**
FRANCIS SCHRODER.
ELEBRED POLHAMUS

No. III. (COPY.)

I, Sarah Potter, residing at Number twenty-three Lombardy-street, hereby certify,

That I was employed by the late Mistress Cary and the late Mistress Snow, on Tuesday, the third day of September, in the present year, to wash some bedding and clothes, which I was informed had, or did belong to some person who had been sick, and that I accordingly did wash one rose blanket, one bed quilt, one pair of socks, one handkerchief, one waistcoat, and three shirts, and that the shirts were very offensive and very yellow. And that, while I was washing the said clothes, the late George Washington Mott came near to the washing-tub, and inquired of me whether I took in washing.

Given under my hand, this 8th day of December, 1822.

(Signed) SARAH POTTER.

On two occasions, it seems that infected clothes or bedding may be peculiarly mischievous, viz. when they are first opened in a warm atmosphere, after having been long closely packed up; and again, when they are put into warm water, for the purpose of washing. The profuse vapour of warm water seizes the matter of infection with the utmost avidity, and conveys it, in its most active state, to the noses, mouths, and lungs of the bystanders. This fact was shown in a very striking manner; for, at the time the clothes referred to in document No. 3, were washing, George Washington Mott, having that day come to the city, was, with Mrs. Cary and Mrs. Snow, standing by the tub: they all sickened with yellow fever on the same day, and all died within a few hours of each other. The infection in Lombardy and Cheapside streets reached to Catherine-street; thus taking possession of three principal avenues which lead to the wharves and the New Market, from the upper part of the city: and to this second infected district, no doubt, we may justly charge some cases of fever which occurred to the eastward and northward of it, and which were not at the time fairly traced: a case in point was that of Mr. D. Rogers. There were, however, three or four cases that fell under my own observation, which were reported to the

Board of Health as yellow fever, in the upper part of the city; aboat which I shall not speak, unless I was disposed to trace a drunken frolic, or something as unlike yellow fever as drunkenness, to the infected district.

I shall conclude with the case of yellow fever that follows, and which was among the most mischievous that occurred in the upper part of the city, during the late autumn, causing more alarm, and consequently more unnecessary removals than any other. This was owing to two reasons; first the supposition of it being impossible to trace it to the infected district; second, it being ascribed to a broken sink in the neighbourhood, a thing which too often happens in like cases. The subject of the case here alluded to, was Mr. Scott, who resided at 122 Bancker-street, by trade a tailor. He had received his work for months by the job from Messrs. Pierson and Jacobs, whose store is at the corner of Maiden-lane and Nassau-street. For wages or work he went there two or three times a-week during August, and until the 10th of September, on which day the store was closed on account of the alarming progress of the fever in the neighbourhood, as several cases had already occurred at the noted sugar-house a few yards distant, which terminated fatally. On this day, the 10th of September, he settled with his employers, but was detained there much longer than usual, owing to some difficulty in his accounts. After receiving his wages, as he stated, on his return home he went through Maiden-lane to Broadway, merely to witness the desolation of that part of the city. Six days after this, to wit; on the 16th of the same month he sickened of yellow fever, and died on the 23d. Another journeyman employed by the same firm, who was at the store on the same day, sickened about the same time that Scott did, and was reported to the Board as a case of yellow fever, but recovered. Mr. Pierson, one of the partners of that firm, however, soon after this took the disease, and it proved fatal.

D. D. WALTERS.

REVIEW.

A TREATISE *on the use of the* PRUSSIATE OF IRON, *or Prussian Blue, in Intermitting and Remitting Fevers.* By WILLIAM ZOLLICKOFFER, M. D. &c. Frederick, Maryland, 1822. pp. 32.

IT has been very much the fashion with a certain class of physicians, to oppose the introduction of any new medicines into our materia medica, upon the ground that the list of our remedies is already unnecessarily extended for all the practical purposes of our profession; that the evils attending an extensive pharmacopœia are numerous and dangerous; "that it renders us incapable of duly estimating the real value and medicinal properties of each remedy in particular;" "that if the truly efficient, but small number of articles of the *armoury of medicine*, are incapable of doing all we wish, it is scarcely to be expected that an extension of their number will add to our means of cure. It is not number, but quality we want, for the few indications which our profession points out to us." In consistence with these views, it is triumphantly asked, "among the *cathartics* can we possibly be deficient, if we limit the number to about a dozen, viz. jalap, rhubarb, senna, manna, castor oil, salts, cream of tartar, sulphur, magnesia, aloes, and, above all, calomel?" "For *emetics*, can we possibly require more than ipecacuanha and emetic tartar, together with two or three others of inferior powers?" and so on of the other classes of medicines. "If, with all these," it is added, "scarcely exceeding half a hundred, we cannot accomplish our intentions, it is much to be feared, success would not follow from a greater augmentation."

These, and sundry other speculations, equally sage and philosophical, were, not long since, formally set forth in a sort of manifesto,* by one of the professors of a neighbouring university. Now although we do not hesitate to admit that our pharmacopœia is susceptible of considerable simplification, yet sorry indeed would we be to rest contented with so barren a list of remedies as that proposed by the learned professor. We will venture to assert, without fear of contradiction, that unless the physician practises according to the most rigid Brunonian principles, he will very frequently find himself at a loss with "the armoury of medicine" here supplied.

Our limits will not permit us to enter upon the many and striking proofs which might be advanced to establish this position. We cannot, however, leave this subject without remarking, that throughout the whole of his reasoning, the writer we have alluded to, goes upon two principles, which are wholly gratuitous. In the first place, he takes it for granted, that our materia medica is already perfect, at least so far as any *additions* to it are concerned. That we are, at present, in possession of all the medicinal agents that can be necessary in the cure of disease, and that the only improvement we are called upon to make is that of retrenchment. This is indeed a bold assumption, when it is recollected that the wisest and greatest men have confessed and regretted the imperfection of medicine. In the second place, he takes it for granted, that the only distinguishing properties of medicines are those which characterize the artificial and arbitrary classes under which they are ranged. For instance, a medicine is either emetic, cathartic, diaphoretic, &c. and if we have one or two powerful articles in each class, we want no more to enable us to fulfil every necessary indication. This is certainly a most limited and untenable view of the operation of medicines. So far from this being correct, scarcely any two articles, even in the same class, will be found to possess similar properties. Every purgative is not equally proper, where catharsis is indicated, nor every emetic equally salutary, where vomiting is called for; on the contrary, one article may be decidedly injurious, while another, of the very same class, may be eminently beneficial. From this it follows, that so far from restricting ourselves to our present knowledge of medicinal agents, we should be stimulated to the discovery of those hidden treasures, which, we doubt not, are still reserved for the benefit of suffering humanity.

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*Cursory Remarks in relation to the objects of the Materia Medica and Pharmacy. By John Redman Coxe, M. D. Philadelphia Journal of the Medical and Physical Sciences, vol. i. p. 34.

As an illustration of the advantages to be derived from an extensive pharmacopœia, we do not know that we can select any thing more striking than the class of tonics, and especially in their application to the cure of intermittent fever. It must have occurred to every practitioner of any experience, to have met not unfrequently with cases of this species of fever, in which the ordinary tonics completely failed to arrest the progress of the disease, which nevertheless yielded to others even of a simpler character and of milder virtues. Peruvian bark, for example, has failed where serpentaria and camomile flowers have proved successful. A pointed instance of this kind may be found recorded in Hillary's Observations on Barbadoes, (p. 92.) If then, from some peculiarities in the disease, or in the constitution of the patient, or from any other cause whatever, it is found that one intermittent fever yields to bark, while another, apparently similar in character, requires a different tonic to check its career, it proves most conclusively, not merely the propriety, but the absolute necessity of having a varied and extended list of tonics, to which recourse may be had in cases of emergency, or in the failure of those more commonly in use; and it exposes most decisively that wretched system, which would fritter down the whole materia medica to a number of medicines, "scarcely exceeding half a hundred."

Entertaining such views, we have been very much gratified in finding our list of tonics augmented by the addition of an article, which, according to the representations of Dr. Zollickoffer promises to be extremely valuable; we mean the *Prussiate of Iron.*

Our author gives the following account of the manner in which he was first led to its use.

"The circumstance that first led to the discovery of the invaluable virtues of the *Prussiate* of *Iron*, as a remedy of very great importance, and worthy of the particular regard and attention of practising physicians generally, was such as might be supposed scarcely entitled to notice, but not unfrequently, occurrences apparently unworthy of the slightest observer, become of the most interesting and important nature to mankind.

"In the latter part of the summer and fall of 1821, more cases of intermitting and remitting fevers claimed my attention, particularly among children under two years of age, than I recollect seeing, during an extensive practice, since I first entered into the laborious and arduous duties of the profession. And this kind of patients claimed my peculiar sympathy, regard, and attention, in consequence of my being fully aware of the existing difficulty of treating them successfully, should the disease not yield to the free and discretionary use of emetics and cathartics; inasmuch as I was confident that barks could not, in the generality of cases, be given in doses suitably large, to produce its revolutionizing effects upon the system, from the circumstance of the exceedingly unpleasant and peculiar taste accompanying its internal exhibition, which I knew to be a very frequent cause of their being refused by the little sufferers, and if taken into their extremely delicate stomachs, of their producing irritation and causing them to be rejected. And being thus situated for want of a remedy equally prompt and effectual with the cort. peruvi. by which I might, with perfect ease, control the unruly and distressing paroxysms of intermitting and remitting fevers; and supposing that nature was not so limited in her gifts as to have provided but one remedy, and that from the vegetable kingdom, for the removal of a disease so general as the one under present consideration, I concluded, after reflecting upon a number of articles that might be supposed, though they had never been recommended, to be productive of some practical utility, to give the Prussiate of Iron a fair trial. I had at this time seven cases in which I exhibited this remedy, and the result of which exceeded my most sanguine expectations; for five out of the number were relieved, without being subject to an attack of the disease for some time."—pp. 11, 12.

Dr. Z. next proceeds to compare the Prussiate of Iron with the other preparations of that metal, with arsenic and Peruvian bark, and as might naturally be expected from the enthusiasm of a supposed discoverer, gives it the decided preference over all of them.

"The *sulphate* and *carbonate* of *iron* have long since been noticed by most writers on the materia medica. These articles certainly possess virtues, as medical remedies, that render them worthy of attention, and deserving of the place which they have obtained in the list of tonics: though from experience, I can boldly assert, that neither of them can be considered of the least importance, in a practical point of view, when compared with the *Prussiate of Iron.* The two former articles, as tonics, are slow in producing the effects peculiar to this class of remedies, upon the human system, and from this circumstance, the result arising from their administration is seldom visible under two or three days: neither have I found either of them competent to the cure of *intermitting* and *remitting fevers.* The *Prussiate of Iron* in this respect, differs very materially from these preparations, in being more certain. prompt, and effectual, in fortifying the subject of this distressing malady against the return of future *paroxysms.*

"The *oxide* of *arsenic* exhibited in the form of the sol. arsen. Fol. min. is an article that is highly prized by many practitioners, on the account of its supposed superiority as a remedy, over all others, in the treatment of intermittents. The utility of this compound cannot be doubted; although the consequences resulting from its internal exhibition are not unfrequently of such a nature as to render the further use of medical aid essentially necessary, in order to relieve the patient from a partially diseased state, that has been brought on by giving it in doses rather large, as likewise by continuing its use for too great a length of time, without occasionally suspending it. Another inconvenience occurs, which would appear to be an objection to its use under all circumstances, and that is, of administering it to the more illiterate class of society, many of whom are of the opinion, that if a small quantity of a medicine is calculated to be beneficial, an increased proportion must be productive of the most certain and happy effects: and by this mode of false reasoning, they are induced to give as much more (out of the phial that may be left them) as their judgment may, on the occasion, influence them to conceive necessary, without having any reference to the rule prescribed by the physician for their observation. Every practising physician of experience in the profession must certainly be well acquainted with this circumstance. inasmuch as they have, from time to time, in paying their visits to the class of people alluded to, discovered this disposition.

The *Prussiate* of *Iron*, which is recommended in this disease, cannot be objected to on this account in consequence of its mildness, and indeed, it is perhaps more so than any other remedy of equal power, with which the present improved and considerably enlarged state of the materia medica acquaints us. It is far more certain, and better adapted to the treatment of intermittents, than arsenic under any form: and it possesses an advantage over this article, in any period of the disease, being a suitable time for its administration; though it has never been productive of nausea even in one solitary instance, where the stomach would not receive, without becoming irritated, every other remedy. I have merely to observe in closing this part of the subject, that it is hoped, that those who are in the constant habit of using *arsenic* will, at least, if not continue to use the *Prussiate* of *Iron*, give it a few trials, in order to test the superior and decided advantages that it has been stated to possess over this article."—pp. 13, 14, 15.

Over cinchona he asserts it to have the following advantages, as a remedy in intermitting and remitting fevers:

"First.—It is void of taste, and may therefore be much more readily exhibited than the *cinch. offic.* which, to some, is extremely unpleasant.

"Secondly.—It may be given in every stage of the disease; while the administration of Bark is confined to the *apyrexiæ.*

"Thirdly.—The dose is much smaller, being from four to six grains twice or thrice in twenty-four hours; or at morning, noon, and at night: while bark, to be effectual, must be given in much larger doses.

'Fourthly.—It never disagrees with the stomach, or creates nausea, even in the most irritable state of this *viscus*: while bark is not unfrequently rejected.

"Fifthly.—In its effects as a remedy calculated to prevent the recurrence of future *paroxysms*, it is more certain, prompt, and effectual than the justly celebrated *cort. peruvian.*

"Sixthly and lastly.—A patient treated with this article will recover from the influence of intermitting and remitting fevers, in the generality of cases, in much less time than is usual in those cases in which bark is employed. In making use of the Prussiate of Iron as a remedy in disease, care must be taken to select that which is of a very dark blue, approaching to a black, having a shining coppery fracture, and adhering firmly to the tongue."—pp. 18, 19.

Dr. Z. concludes his account of the Prussiate of Iron, by the detail of several cases illustrating its success in the cure of intermitting and remitting fevers.

We cannot close this notice of Dr. Zollickoffer's book without correcting an error into which he has fallen, (unconsciously we are convinced,) in supposing himself the first person who ever used the Prussiate of Iron as a medicine. He says, this article "is considered by limners, to be a paint of much value, by combining it in certain proportions with other substances; but as an article worthy of regard and attention, *on account of its application as a medicine, calculated to alleviate the sufferings of fallen humanity, induced by disease, it has never been known.*"—p. 7.

And again in his preface:

"That this remedy will at first meet with many opponents to its general introduction, I am well aware of, knowing that *all new discoveries* have, with considerable difficulty at first, overcome the unfavourable opinions respecting their utility, as applicable to disease."

By turning to Lewis's Dictionary of the Materia Medica, 4th edition, by Dr. Aikin, Dr. Z will be convinced that he has long since been anticipated. Under the article Iron it is stated that "some have made trial of the blue precipitate called *Prussian blue*, and report that it seemed to act as a diaphoretic, and in some cases as an aperient.* Of all the known preparations of iron," it is added, "this promises the least activity: the perfect calces, almost if not wholly inert, are soluble in certain acids, particularly in the marine, but the Prussian blue is not acted upon by any kind of acid menstruum." To the credit of having discovered and established its tonic virtues, we believe Dr. Z. is fully entitled, and the still higher credit of having successfully applied it to the cure of a very common, and not unfrequently, dangerous class of diseases.

Medical and Philosophical Intelligence.

DOMESTIC.

Notices *of some of the things received into my Museum and Library, within a short time.* Addressed to the Editors of the New-York Medical and Physical Journal. By Samuel L. Mitchill, M. D. Professor of Materia Medica: Dec. 24, 1822.

From the region inhabited by the Caraib and Curaru Indians, toward the sources of the river Oronoko, several hundred miles above the city of Angostura, arrives, by the kindness of Dr. Forsyth, a quantity of the deadly Curaru or Worraroo poison, prepared in those parts by the natives. There is a quiver full of darts, whose points have been dipped in this terrible venom. The shafts are furnished with cotton instead of feathers, to give them direction. They are conveyed to their mark, whether a bird, a beast, or an enemy, by being blown through a reed or other hollow tube.—There is also a small calabash, containing a considerable quantity of the formidable article, for further use and satisfactory experiments. It is reported to be prepared from certain plants by decoction and extract.

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* Geoffroy. Memoires de l'Acad. Roy. des Sciences de Paris, pour l'ann. 1743.

By the generosity of Captain Ridgeley of the U. S. Navy, comes to hand the fœtus of a *cetaceous* animal, taken alive from the uterus of its dam, off Cape Horn. It probably belongs to the *Delphinus rostratus.* This unborn creature is furnished after the manner of land animals with an umbilical cord and umbilical vessels. Young as the individual is, the teats situated near the vulva, one on each side, sufficiently distinguish the sex. From these parts of the organization, in addition to many others, a conclusive judgment may be formed against the vulgar error that a whale is a fish.

The cryptogamic vegetable, from the dreary land newly discovered, beyond Cape Horn, and brought by Captain Napier, turns out, after careful examination by John Torrey, M. D., to be an undescribed species of Roccella. It is in the shape of moss, and in good condition for botanical research. A particular description, with a correct drawing, has been finished by that gentleman, for the purpose of publication. It is a curious subject for reflection that, as the navigators say, the whole upland Flora of New South Shetland consists of a single species.

The *mytilus lithophagus*, or *stone-eating mussel*, presents himself in the cavities of the calcareous rock, which he has penetrated and inhabited, along the shores of the Mediterranean sea, near Genoa. The specimens are in fine order, and present the interesting spectacle of a bivalve mollusca with a delicate shell, perforating the solid limestone, and dwelling in the cavity he forms for himself. By what means he makes the excavation, whether by some mechanical instruments, or some chemical menstruum, is a matter upon which naturalists have not as yet decided.

The skull and lower jaw of the Royal Tiger of Bengal, with the set of teeth complete, are fine pieces of osteology. The ferocious beast was shot in the neighbourhood of Calcutta by a person upon whom he was about to spring.

I received from Lima the Spanish publication of the distinguished physician, Don Balthazar de Villalobos, on a remarkable epidemic fever which affected various districts of the department of Chancay in Peru, during 1796 and 1797. The book was written pursuant to an order of the governor and the royal court of justice, and is dedicated to the viceroy Ambrosio O'Higgins, and is highly recommended by Don Hipolito Unanue, Professor of Anatomy in the College of St. Fernando, &c. The distemper is reported to belong to the class of acute pyrexiæ, known by the names of *causos* and *putrid fevers.* It sometimes attacked the lungs, pleura, fauces, and brain, and thereby acquired higher malignancy. But the elevated grade of the disorder was produced by dissolving the cohesion, disturbing the equilibrium, and changing the figure of the fluids and humours of the body, thereby destroying their crassis and disorganizing their constitution. This disease, by reason of its violence and mortality, was considered pestilential. It was alleged to have had its origin in Calpa, and thence proceeding a distance of ten leagues, without invading the intermediate population, to have fixed itself in the village of Andalusia, and the neighbouring towns of Huara, Begueta, Mazo, Luriama, and Huacho. The centre of the mortality was almost thirty leagues from Lima. Its *chief victims were the native Indians, the slaves of the agency;* the free persons who suffered were very few. So serious and alarming were the effects, that a consultation, consisting of four eminent physicians, was deputed by public authority to visit the sickly district, and to adopt measures for relieving the sufferers, and of arresting the progress of the malady. Their names were Aguirre, Bueno, Delarocha, and Villalobos ; the last of whom is the writer of the work under consideration. It contains upwards of 150 pages in small 4to. and by reason of the facts and cases it contains, is a very instructive performance.

Another dissection has been made by Dr. W. Anderson and myself of the *Siren lacertina* of the southern states. The individual was received from John P. Thomas, M. D. of Pineville in South Carolina. No doubt can be entertained that this reptile possesses both *gills and lungs.* It seems to be fully established that the specimens we have had for examination are not larvæ, but perfect animals. There were three principal divisions of the gills on each side of the neck, and three open holes communicating between the external fluid and the throat. The arches have short pectinated processes, like those in fish. They are situated on the under side of the arches, and point toward the throat. Thus ambient air or water seems therefore to pass through and between the arches exactly as in fishes: the fringes on the outside, which have by some observers been considered as the breathing organs, may now be considered as merely opercula or gill-covers; and this opinion is countenanced by our failure to inject their vessels with quicksilver. Notwithstanding this branchial organization, we found within the animal extensive and capacious lungs. These commenced by bronchial tubes from the trachea behind the heart, and extended through the abdomen as far as the cloaca, one on each side of the spine. Upon being inflated, they exhibited a cellular structure. At their origin they were of small capacity; they enlarged toward the middle; they contracted toward their posterior terminations; and ended each in a point very near the vent.

FOREIGN.

Description of an Apparatus for removing Poisons from the Stomach, invented by Mr. Jukes, Surgeon.—We might with justice be considered to be forgetful of the duty we owe the Profession, were we to fail in laying before them a description of a very excellent apparatus which Mr. Jukes, its very ingenious inventor, favoured us with a sight of. It consists of an elastic gum tube, a quarter of an inch in diameter, and two feet and a half in length, terminating at one extremity in a small globe of ivory, with several perforations; the other extremity is adapted, either by screw or by plug (the latter is preferable) to an elastic *bottle* of sufficient size to contain at least a quart of liquid, and having a stop-cock fitted to it, in a similar manner as in the hydrocele bottle. Instead of the *bottle*, a pewter *syringe*, of an equal capacity, may be adapted, in the same manner, to the flexible tube. The operation by the syringe is performed more quickly, and may therefore, perhaps, be preferred by some. In cases where surgeons have neither *bottle* nor *syringe*, the *tube alone* might be made to answer the purpose, if the operator apply his mouth to the extremity, and thereby institute the office of a siphon.

Application.—The patient ought to be placed on the left side, and the *globulated* end of the tube be then carefully passed to the greater curvature of the stomach, either through the mouth or nostril, as may be thought proper. Having previously filled the *bottle* or *syringe* with warm water, at the temperature of 150°, screw or plug it to the tube, turn the stop-cock, and gently force the contents into the stomach. The then diluted contents are to be immediately withdrawn by pulling up the piston ; or, if the *bottle* be applied, the same effect will ensue from its elasticity enabling it to recover its original form, by which the fluid contents will return, charged with the poison. This operation ought to be repeated, till the water, which is withdrawn, becomes clear and tasteless.

In Mr. Jukes' experiments, first on dogs, and then on himself and others, assisted by Mr. James Scott, Surgeon, in Westminster, the apparatus was proved fully to answer the intended purpose. In these experiments, Mr. Jukes swallowed, first *two drams* of laudanum ; he afterwards gradually increased the quantity, until it reached ten drams : since which, he has administered to several individuals (one of them a female) *one* ounce of laudanum, with an equally successful result. The utility of so well contrived an instrument, arising especially from its being equally adapted to the removal of all the more bulky poisons, must be evident to every one. We consider that Mr Jukes has rendered an essential service to the profession, and to the community, by its invention.—*Lon. Med. Repos., Oct.*, 1822.

Presence of Urea in the Blood.—M. Ségalas, (with Professor Vauquelin) has lately discovered urea in the blood of dogs which had been deprived of their kidneys ; confirming what M.M. Prévost and Dumas, of Geneva, had previously announced.—*Bulletin des Sciences par la Societe Philomatique, &c. Paris. Juin*, 1822.

Test for Magnesia.—In the *Annales de Chimie* for May last, it is tated, that if we spread any clear liquid upon a plate of glass, and then trace the word *Magnesia* upon the glass so covered, with the end of a glass rod, that word will appear in *white characters* if magnesia be present in the solution; if not, no such appearance will result. It is, moreover, stated, that this method of discovering the presence of magnesia originated with Dr. Wollaston, and was communicated to the Editors by M. Clement.

In the same journal for July, M. Clement writes as follows to the Editors:

"Gentlemen,—In your number for May last you have given a very inaccurate report of a neat experiment which Dr. Wollaston was so kind as to show me when in London. The precipitation upon the written characters only ensues when the magnesia solution has been previously decomposed by a mixture of phosphate and carbonate of ammonia. This substance, re-dissolved by the excess of carbonate, is precipitated upon the traces of the glass rod, in consequence of the expulsion of carbonic acid by the heat disengaged by friction."—*Brande's Journal of Science, No.* 27.

Detection of Poisons.—A paragraph has appeared in the papers, recommending blue sugar-loaf paper as a test of distinction between oxalic acid and Epsom salt; it is reddened by the former, but not affected by the latter. This is perfectly true; but a simpler test consists in wetting the tip of the finger, applying it first to the supposed salt, and then to the tongue—if oxalic acid, it tastes *very* sour; if Epsom salt, very bitter and saline.

Many restrictions have been suggested upon the sale of arsenic; the only effectual bar to the mischief that results from it, is *prohibition.* It is of no use in medicine, and should therefore be rejected from the pharmacopœia. Apothecaries and druggists need not then keep it. It is sometimes employed as an instrument of research, in the chemical laboratory, which might be sufficiently supplied through other channels.—*ibid.*

New Blowpipe.—Professor Green has published an account of a new Blowpipe in the *American Journal of Science*, vol. iv. p. 164. The principle, however, is not new; and an instrument almost exactly similar to it was published by its inventor, John Farey, Esq. *junior*, in the article *Blowpipe*, in the *Edinburgh Encyclopædia*, vol. iii. p. 615.—*Edin. Philosoph. Journal, No.* 12.

On the difference between Sea and Land Air.—Having learned that on the Baltic, asthmatic invalids were much better at sea than on shore, M. Vogel analysed the air a league from the shore, and concluded, 1. That the air above the Baltic, a league from the shore, contains less carbonic acid than the ordinary atmosphere, and carbonic acid probably diminishing as we recede from the land; and, 2. That the same air contains muriates in greater or lesser quantities.—*Ibid.*

State of the Skin, &c. in Yellow Fever.—From the facts observed by himself, or recorded by others, Dr. Desmoulins has been led to the following conclusions with regard to the biliary secretion and yellowness of the skin:—

1. That, in yellow fever, there is no increase of the secretion of bile.
2. That the black vomit and dark-coloured stools are the produce of exhalation from the surface of the intestines.
3. That the yellow colour of the skin is produced by the blood in the minute vessels of the cutis, which are in a state of congestion, analogous to that which, in the mucous membranes, gives rise to hemorrhage.
4. That the firmer texture of the skin alone prevents this likewise from ending in a similar discharge.
5. That the yellow colour, which is almost always preceded by petechiæ, is only a sort of general ecchymosis.
6. That the yellow fever is nothing else than a congestion or determination of blood to the skin and mucous membranes simultaneously, principally to those of digestion.—*Lond. Med. and Phys. Journal, Oct.* 1822.

Treatment of the Barcelona Fever.—Dr. Francais has read to the Academy of Medicine a memoir on this subject. Blood-letting, which has been frequently employed and regarded as highly useful in America, has always proved deleterious in Spain. The tonic and revulsive treatment has succeeded best, particularly the exhibition of cinchona, and the application of the moxa along the spinal column. It may readily be supposed that the means of employing these universally did not exist, but, according to Dr. Francais, the trials made were sufficient to demonstrate the happy effects resulting from this plan..—*Ibid.*

Effects of drinking Boiling Water.—It is the custom of some poor and thoughtless persons to suffer children to drink from the spout of a tea-kettle while filling it at the pump; they have afterwards attempted to drink when it has just been taken from the fire, supposing it still to contain cold water. No less than four cases of this kind are related in the *Medico-Chirurgical Transactions*, by Dr Hall. The symptoms produced are those of *croup*, that is, of inflammation of the glottis and larynx, and not, as might have been expected, of the œsophagus and stomach. It appears, indeed, probable, that the boiling water does not actually reach the stomach, but that it is arrested by spasm of the pharynx. Dr. Hall recommends an incision into the windpipe, but the only case of this operation which he relates, proved, as might have been expected, fatal. Where the injury is extensive, there seems to be no remedy.—*Brande's Journal of Science, No.* 27.

Cure of Ring-worm.—Mr. T. J. Graham, of Cheltenham, recommends the lime-water which has been used for purifying gas, as a very efficacious remedy in the above troublesome disease. The head is to be well cleansed morning and evening with soap and water, and afterwards washed with the lime-water from the gas-works. (*Monthly Mag. Sept.*) The above *lime-water* is a very heterogeneous compound, so that it is impossible to say which of its ingredients is effectual. It contains lime, ammonia, sulphuretted hydrogen, volatile oil, and, probably, several other compounds of a more complex nature.—*Ibid.*

Mineralogy.—A work on the science of mineralogy is about to make its appearance in Germany. It is from the pen of Mr. Frederick Mohs, Professor of Mineralogy at Fryberg, and will contain the terminology, the rules of the construction of Mr. Mohs' system, and the nomenclature, the characteristic and the descriptive part of natural history. The whole to be comprised in two volumes octavo, with plates.—An English translation will appear at the same time, made under the inspection of the author, by Mr. Haidinger, who lately visited this country, in company with Count Brenner. From the known celebrity of the Fryberg school as well as the acknowledged ability of the author, we have reason to expect that this will be a useful work.—*Ibid.*

Experiments on Incubation.—M. GEOFFROY SAINT-HILAIRE has communicated to the Institute of France an interesting account of different methods by which *oviparous* animals may be rendered *viviparous.* These experiments completely succeeded with regard to water-snakes (*couleuvres acquatiques,*) when placed in a dry situation, and under circumstances unfavourable for dropping their eggs. These were retained by the parent, and preserved in the genital receptacle ; so that, at the end of a certain period of this uterine incubation, the young came forth from the eggs and the body of their mother. These experiments were not followed with equally successful results in those animals whose eggs have calcareous shells. Notwithstanding his endeavours, M. Geoffry has not succeeded in effecting the production of young chickens alive ; but he is led, from his observations, to believe that this might be done by better contrived means.

Rudiments of a Fœtus in the Testicle.—Dr. FRIEDLANDER relates that a child was born in December, 1817, at a village near Glogau, which at first appeared in all respects healthy and natural. In the month of May following, it was seized with difficulty in making water, and the right testicle began to enlarge. By the 19th of June, the testicle had so much increased as to descend to the knee. On the 19th of July, a ligature was applied near the ring, which came away on the 22d, and the child did well. The testicle was four inches three lines (Rhine measure) in length, two inches four lines in breadth ; its weight, seven ounces ; the epidydimis was entirely wanting. After careful examination of the tunica vaginalis, a hard body was perceived, which proved to be an os femoris, eighteen lines long, and without periosteum. Several other bones were subsequently found, connected, by means of cellular texture and muscular fibres, in such a manner as to form the pelvis and right lower extremity of a fœtus about four months old. The left branch of the os pubis and os ischium were wanting ; the ileo-femoral articulation presented a triangular appearance ; the sacrum was distinct, with its surface for receiving the first bone of the spinal column ; in the middle of the pelvis was a ligamentous mass, about an inch long, resembling the rudiments of the lumbar vertebræ. The tibia and fibula were naturally formed ; the interosseous ligament " a little too thick."

METEOROLOGICAL OBSERVATIONS

FOR SEPTEMBER, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer			Winds.			Rain Guage.	Hygrometer.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 A M	2 P M	9 P M			
1	30 37	30 30	30 22	70	85	74	se	se	se	—.08	685	variable
2	30 10	30 04	30 00	76	81	70	s	s	nw	—.90	724	cloudy
3	29 90	30 00	30 00	67	87	74	sw	sw	sw		704	variable
4	30 08	30 20	30 20	66	82	67	sw	sw	sw		684	clear
5	30 26	30 26	30 26	62	81	69	sw	sw	sw		656	do
6	30 26	30 26	30 26	65	82	71	ne	sw	sw		645	do
7	30 26	30 26	30 15	68	88	76	sw	sw	sw		636	do
8	30 15	30 15	30 15	70	88	75	nw	sw	s		638	cloudy
9	30 10	30 10	30 22	72	85	76	sw	s	s		672	do
10	30 20	30 20	30 20	70	90	76	s	s	sw	—.01	690	clear
11	30 14	30 14	30 14	75	94	78	sw	sw	sw		687	cloudy
12	30 27	30 27	30 27	76	91	77	sw	sw	sw		660	clear
13	30 27	30 27	30 27	78	94	79	sw	sw	nw		684	clear
14	30 27	30 20	30 20	72	84	72	sw	se	sw	—.31	695	cloudy
15	30 14	30 14	30 00	74	90	74	sw	sw	nw		670	clear
16	29 90	29 90	30 10	72	82	69	sw	nw	nw		638	do
17	30 30	30 44	30 56	60	68	60	nw	nw	sw		622	do
18	30 64	30 60	30 52	54	71	62	nw	sw	sw		619	do
19	30 38	30 38	30 30	64	79	68	sw	sw	nw	—.38	660	cloudy
20	30 00	30 00	30 00	69	75	69	sE	nw	nw	1.62	680	do
21	29 90	30 00	30 12	65	72	62	sw	sw	nw		650	clear
22	30 36	30 36	30 40	58	70	62	nw	nw	nw		645	do
23	30 57	30 57	30 57	55	70	60	nw	nw	nw		642	do
24	30 50	30 50	30 50	58	73	62	nne	ne	sw		648	do
25	30 44	30 44	30 44	59	74	68	cal.	sw	sw		650	do
26	30 44	30 30	30 30	64	75	73	sw	sw	sw	—.07	718	cloudy
27	30 30	30 16	30 16	70	77	74	sw	s	s		800	do
28	30 16	30 16	30 00	73	83	75	s	se	se	—.03	832	do
29	30 00	30 00	30 00	73	78	72	s	se	se		834	do
30	30 00	30 00	30 12	68	77	70	nw	nw	nw		750	clear

The quantity of rain in this month, amounted to 3 inches and 40—100ths.

The thermometrical range for September was considerably higher than for the same month in the preceding year; it was unusually moist, especially towards the latter part. Very thick fogs and light winds, on the mornings of the 3d, 9th, 24th, and 26th. The autumnal equinox made its appearance at 10 P. M on the 19th, and continued until the evening of the 20th. The sun was obscured the greatest part of the 14th, and completely so on the 20th, 26th, and 27th. The thermometer, placed in the sun, on the 6th, at 2 P. M. stood at 106°. Thunder, lightning, and rain, on the 2d and 14th. Vespertive lightning on the 11th and 13th. The rain on the 1st 10th, and 19th, fell in the night.

METEOROLOGICAL OBSERVATIONS

FOR OCTOBER, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer.			Winds.			Rain Guage.	Hygrometer.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 AM	2 PM	9 PM			
1	30 [illegible]	30 27	30 [illegible]	56	[illegible]	66	nw	nw	nw		710	clear
2	30 30	30 30	30 46	53	58	54	nw	ne	ne		696	cloudy
3	30 47	30 40	30 36	51	62	[illegible]	ne	ne	ne		690	variable
4	30 30	30 30	30 30	[illegible]	72	[illegible]	ne	ne	ne		687	clear
5	30 30	30 30	30 30	58	73	73	ne	ne	ne		693	do
6	30 30	30 30	30 30	[illegible]	[illegible]	61	ne	se	se		710	do
7	30 [illegible]	30 [illegible]	30 10	[illegible]	78	68	se	se	se		724	cloudy
8	30 08	30 08	30 08	65	80	65	nw	nw	nw	—.07	730	clear
9	30 20	30 [illegible]	30 13	65	73	[illegible]	nw	sw	sw		730	do
10	30 [illegible]	30 15	30 [illegible]	67	75	70	sw	sw	sw	—.11	735	cloudy
11	30 01	30 00	30 00	70	73	72	sw	sw	sw	—.03	780	do
12	30 [illegible]	30 00	30 13	[illegible]	74	61	sw	sw	sw		825	do
13	30 [illegible]	30 [illegible]	30 30	[illegible]	74	[illegible]	sw	sw	sw		770	clear
14	30 [illegible]	30 [illegible]	30 [illegible]	[illegible]	66	56	ne	ne	ne		732	cloudy
15	30 [illegible]	30 [illegible]	30 45	[illegible]	63	[illegible]	nw	nw	nw		[illegible]	clear
16	30 45	30 40	30 33	52	70	62	se	w	w		672	cloudy
17	30 22	30 [illegible]	30 20	56	73	[illegible]	w	w	sw		682	clear
18	30 20	30 [illegible]	30 10	[illegible]	[illegible]	[illegible]	sw	sw	sw		687	cloudy
19	30 10	30 [illegible]	30 [illegible]	63	[illegible]	70	sw	sw	sw		725	do
20	30 00	29 97	29 86	[illegible]	[illegible]	[illegible]	sw	sw	sw	—.02	732	do
21	29 75	30 [illegible]	30 [illegible]	[illegible]	[illegible]	[illegible]	s	nw	nw		686	do
22	30 26	30 [illegible]	30 40	[illegible]	[illegible]	[illegible]	nw	nw	nw		[illegible]	clear
23	30 40	30 [illegible]	30 40	37	58	[illegible]	n	n	n		657	do
24	30 [illegible]	30 34	30 [illegible]	43	[illegible]	[illegible]	n	n	n		632	cloudy
25	30 20	30 [illegible]	30 [illegible]	[illegible]	[illegible]	[illegible]	ne	ne	ne		623	do
26	30 40	30 [illegible]	30 [illegible]	[illegible]	[illegible]	[illegible]	ne	ne	ne		622	clear
27	30 35	30 [illegible]	30 01	[illegible]	[illegible]	46	ne	ne	ne	1. 42	610	cloudy
28	29 [illegible]	29 [illegible]	29 60	[illegible]	[illegible]	46	ne	nw	nw	—.67	655	do
29	29 [illegible]	29 80	29 87	[illegible]	[illegible]	45	w	w	w		650	do
30	29 75	29 90	30 03	[illegible]	[illegible]	48	w	w	w		650	clear
31	30 05	30 [illegible]	30 [illegible]	42	68	56	sw	sw	sw		650	do

The quantity of rain which fell this month, is 2 inches and 32—100ths

The weather of the month of October, was, for the most part, pleasant and dry. The nights were generally clear and starry. On the mornings of the 4th and 18th, there was heavy dews, with light winds. The sun was obscured on the 2d, 11th, 14th, 27th, and 28th. On the 22d there was frost at the villages of Bloomingdale and Hoboken, a few miles from this city, but the thermometer, it will be seen, stood at 37° on the same morning, in a S. E. exposure, at 7 o'clock. The rain, on the 8th, fell a short time before sunrise.

METEOROLOGICAL OBSERVATIONS

FOR NOVEMBER, 1822.

Made at the New-York Hospital, by RICHARD PENNELL, M. D.

	Barometer.			Thermometer.			Winds.			Rain Guage.	Hygrometer.	Weather.
	7 A M	2 P M	9 P M	7 AM	2 PM	9 PM	7 AM	2 P M	9 PM			
1	29 90	29 94	30 05	44	51	44	nw	nw	w		644	cloudy
2	30 05	30 05	30 00	38	54	46	w	w	w		638	variable
3	30 00	30 18	30 34	44	61	45	w	w	n		635	clear
4	30 44	30 44	30 34	36	60	43	n	n	n		630	do
5	30 44	30 44	30 38	35	58	48	n	n	n		632	do
6	30 30	30 28	30 28	44	57	51	nw	nw	nw		645	cloudy
7	30 28	30 22	30 16	49	59	56	nw	sw	sw	—.66	682	do
8	30 04	30 18	30 34	56	75	60	sw	sw	sw	—.13	695	do
9	30 48	30 50	30 56	44	51	45	ne	ne	ne		687	do
10	30 67	30 67	30 64	40	50	49	ne	ne	ne		690	do
11	30 50	30 46	30 32	48	60	56	ne	ne	ne		710	do
12	30 30	30 30	30 30	51	60	58	se	se	se		758	do
13	30 30	30 18	30 08	56	49	48	ne	ne	ne	2.80	758	do
14	30 00	29 84	29 98	48	51	42	ne	nw	nw	—.57	763	do
15	30 25	30 36	30 42	37	58	46	nw	nw	sw		746	clear
16	30 48	30 48	30 48	42	61	50	sw	sw	sw		734	do
17	30 48	30 48	80 40	41	63	42	sw	sw	sw		728	do
18	30 36	30 36	30 29	45	51	47	ne	ne	ne	—.78	742	cloudy
19	30 29	30 29	30 34	45	54	47	nw	nw	nw		741	variable
20	30 34	30 30	30 26	44	58	50	nw	nw	nw		741	cloudy
21	30 20	30 20	30 14	50	61	55	sw	sw	sw		741	do
22	30 00	29 84	29 70	46	47	14	n	ne	ne	1.94	746	do
23	29 60	29 98	30 08	44	57	44	w	w	w		735	do
24	30 20	30 38	30 56	40	46	40	nw	nw	nw		716	variable
25	30 56	30 30	30 26	35	59	44	nw	sw	sw		727	do
26	30 26	30 26	30 26	36	42	39	nw	nw	nw	—.20	710	do
27	30 26	30 26	30 26	34	45	40	nw	w	w		705	clear
28	30 16	29 90	29 98	40	53	50	s	s	s	—.36	736	cloudy
29	30 00	30 00	29 95	47	58	45	w	w	w	—.10	765	do
30	29 90	29 84	29 84	56	69	61	s	s	s	—.31	774	do

The quantity of rain which fell this month, was unusually great, being 7 inches and 85—100ths.

The 8th was very sultry, with vespertine lightning, and rain, at 9 P. M The grass and house-tops were white with hoar frost on the mornings of the 4th, 5th, and 17th. On the 18th, moderate rain all day, with lightning in the S. W at 7 P. M. The sun was partially obscured on the 29th and 30th, and totally on the 7th, 10th, 11th, 13th, 14th, 18th, 21st, and 22d. On the 26th the sun was overclouded a short time after it had risen; rain soon commenced, intermingled with large flakes of snow, which instantly melted when they came in contact with the earth: it continued until 11 A. M. when a clear afternoon and a starry night succeeded.

Obituary.—Died, on Thursday the 5th of December 1822, of Phthisis Pulmonalis, Jacob Dyckman, M. D. Commissioner of Health, Surgeon to the New-York Alms House, Secretary of the Literary and Philosophical Society of New-York, Member of the New-York Historical Society and of the Lyceum of Natural History, and one of the Editors of the New-York Medical and Physical Journal.

In recording the death of their beloved and lamented Colleague the surviving Editors cannot, in justice to their feelings, withhold the tribute of their testimony to his worth.

Jacob Dyckman was born in the County of West Chester, in 1788, and being early destined for a liberal profession, was carefully prepared by the usual preliminary studies for Columbia College, which he entered in the autumn of 1806. While in this distinguished seminary he endeared himself to all by the suavity of his manners and strict propriety of conduct, and particularly gained the good will of his preceptors, by vigorous and well sustained diligence. He graduated in 1810, and after a few months attention to his health, which at this time first became delicate, he commenced his professional studies under the direction of Dr. Hosack. Here he continued to support the high moral character which he had before acquired, with uniform evidences of respect and esteem from his associates.

He graduated as Doctor of Medicine in 1813, in the College of Physicians and Surgeons in the University of the State of New-York; on which occasion he prepared for publication an Inaugural Dissertation on the Pathology of the Human Fluids.

On the merits of this production the public opinion has been distinctly pronounced. It contains, without doubt, the best exposition of some of the doctrines taught in the University of New-York, by the distinguished Professor of the Practice, and has contributed in an eminent degree, to the high popularity which they now enjoy. It manifests, throughout, an extensive and intimate acquaintance with medical literature, and a capacity for analytical investigation, worthy of riper years. In compliance with the advice of his friends and the public expectation, it was the intention of the author to have extended his inquiries, and augmented his work, in a new and improved edition.

A review of this Dissertation contained in the Philadelphia Journal of the Medical and Physical Sciences, thus concludes, after combating the principles it supports: "We have rarely read an early production of the kind, which displays more recondite research, more dexterity of statement, more ingenuity of argument, or plausibility of style and manner. It is an able defence of the cause it espouses, and affords an earnest of future excellence, which no one will more delight to welcome and celebrate than ourselves."—Vol. iv. p. 370.

His constitution again became enfeebled by a pulmonary attack, which, however, was not so severe as to prevent him from entering on the practice of his profession; nor did it interfere with his devotion to pursuits subservient to the cause of humanity. These he continued diligently to prosecute, and in the year 1818, he published an edition of Dr. Duncan's Dispensatory, to which he added many valuable notes, which manifested much botanical research, and a familiar acquaintance with the indigenous medical resources of our country. His attachment to these pursuits was unremitted, and he had been engaged for some time in collecting materials for a Medical Botany of the United States.

He was successively appointed Physician to the New-York Dispensary, Surgeon to the New-York Alms House, and Commissioner of Health; in all which situations the public confidence in his fidelity and zeal, was never for a moment impaired.

At the commencement of the present year, the New-York Medical and Physical Journal was planned, and much was anticipated by the present editors, from the diligence and qualifications of their mutual friend; but there are strong reasons for believing from the great feebleness which almost immediately ensued, that even then the irresistible mandate had been issued which was to call him from all earthly communion.

All who were acquainted with the deceased will delight to dwell on the amenity of his disposition, and the blameless tenor of his life. Remarkably free from the malignant passions, his heart was the seat of generous feelings, and was ever alive to the sensibilities of humanity. In every sphere in which he moved, his worth was confessed; and in every situation to which private confidence or public favour called him, his zeal and industry were incessant and unwearied. He has left behind him many connected by the endearments of friendship: none who can deny the benevolence of his heart, or the purity of his character.

Notice to Correspondents.

In order to insert the interesting communications of Drs. Bayley and Walters, we have been compelled to omit several papers on Natural History and a review of Professor Warren's Treatise on the Nervous System, although we have exceeded our customary limits.—In our next number we shall also notice the late republications of Cooper's Surgery, and the Surgical Dictionary ; the former with notes by Dr. Stevens, and the latter with additions by Professor Anderson.

We have observed that several editors of periodical works are in the habit of selecting original articles from this Journal, and that without any acknowledgment. This conduct is uncandid and illiberal ; and whether the guilt rests with the editors alluded to, or results from the writer's desire to see his essay more extensively circulated, the consequences are the same to us. Hereafter communications forwarded to us must be sent to no other journal; or if copied, it is expected that the obligation will be stated.

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